PROCEEDINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA
FOR PROMOTING USEFUL KNOWLEDGE

VOLUME 86

1943

THE AMERICAN PHILOSOPHICAL SOCIETY
PHILADELPHIA

1943

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PROCEEDINGS
OF THE

American Philosophical Society

Held at Philadelphia for Promoting Useful Knowledge

VOLUME 86, NUMBER 1 SEPTEMBER 25, 1942

THE EARLY HISTORY OF SCIENCE AND LEARNING IN AMERICA

Papers read before
The American Philosophical Society
Midwinter Meeting
February 13–14, 1942

PHILADELPHIA
THE AMERICAN PHILOSOPHICAL SOCIETY
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THE EARLY HISTORY OF SCIENCE AND LEARNING IN AMERICA

With Especial Reference to The Work of the American Philosophical Society During the Eighteenth and Nineteenth Centuries

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Papers read before
The American Philosophical Society
Midwinter Meeting
February 13–14, 1942

PHILADELPHIA
THE AMERICAN PHILOSOPHICAL SOCIETY
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1942

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INTRODUCTORY REMARKS

EDWIN G. CONKLIN

Vice-president, American Philosophical Society

(*Read February 13, 1942, in Symposium on the Early History of Science and Learning in America*)

THIS year marks the close of the second century of this Society which was organized by Benjamin Franklin in 1743 "for the promotion of useful knowledge among the British plantations in America." Therefore, it has seemed fitting to mark this bicentenary by a review of some of the notable work of the Society in various fields of learning, and accordingly different persons were asked to prepare papers on the contributions of the Society in the fields of their special interests.

Further consideration led to the conclusion that it would be more useful to make this occasion a general review of the Early History of Science and Learning in America, with especial reference to the work of this Society during the eighteenth and nineteenth centuries. Such a review would have a broader interest than the work of one institution, and it would have the practical result of assessing the wealth of our library and archives in this field and of learning its principal needs, if it is to be an important research library, as was recommended by the special committee on the library at the General Meeting last April.

Accordingly this program was planned to begin with the mathematical and physical sciences and then to proceed to the geological and biological and finally to the social sciences and the humanities. Such a plan covers an enormous field, too great for any one meeting and it is hoped that many subjects which could not be represented in this program such as zoölogy, anthropology, psychology, sociology, philology, political science, economics, government, jurisprudence, arts and letters may be taken up at some future meeting, perhaps next fall or winter. American archaeology will have its day at the Annual Meeting next April, and a year later it is proposed to devote the General Meeting of 1943 to the celebration of the 200th anniversary of the birth of Thomas Jefferson, who was the third president of this Society, serving in that capacity for eighteen years, during eight of which he was also President of the United States. This Jefferson celebration should also bring out the important part which the American Philosophical Society had in the establishment of our national independence and in the organization of our Government as well as its later development.

This Society has held many anniversary celebrations during its history, but none of the kind which is here proposed. The 100th anniversary of its founding was celebrated May 25–30, 1843, with a comprehensive address on the origin of the Society by one of the vice-presidents, Dr. R. M. Patterson, and a series of eight sessions for the presentation of original papers.

The 100th anniversary of the granting of the Charter and the incorporation of the Society on March 15, 1780, was celebrated exactly one hundred years later in an historical address by President Frederick Fraley and by ten after-dinner speeches of such length and weight that they must have occupied most of the night if given in full as they are printed in the PROCEEDINGS.

The 100th anniversary of the first occupation of the present Hall was held on November 21, 1889, with another historical address by President Fraley and another famous dinner at which five toasts were proposed and responded to as reported in some thirty pages of the PROCEEDINGS.

Again on May 22–26, 1893, there was a grand celebration of the 150th anniversary of the foundation of the Society with addresses by eleven officers, members and delegates and greetings from 95 institutions; technical papers were presented in abbreviated form by twelve scholars in different fields of science, which were published in full in some 650 pages and 58 plates of the PROCEEDINGS.

Finally on April 27–30, 1927, was held the 200th anniversary of the founding of Franklin's Junto, out of which grew one of the constituent societies which united with the American Philosophical Society in 1769—delegates from 130 institutions were present, 32 addresses and papers were read and the celebration closed with a notable dinner and series of toasts and all these features are preserved in a stately volume of the PROCEEDINGS of 750 pages.

But the greatest celebration in the history of this Society was that commemorating the 200th anni-

versary of the birth of Benjamin Franklin which was held April 17–20, 1906. For this celebration the Legislature of Pennsylvania appropriated $35,000.00, but our distinguished and conservative member, Governor Samuel W. Pennypacker, cut this down to $20,000.00. The Congress of the United States ordered a Franklin medal to be struck in commemoration of this event and appropriated $5,000.00 for that purpose. Delegates were present from many foreign nations and from 130 learned societies, universities and institutions. Honorary degrees were conferred by the University of Pennsylvania and St. Andrews University, Scotland, through its Provost Andrew Carnegie, ceremonies were held at the grave of Franklin. Notable addresses were given on Franklin as Citizen and Philanthropist, as Printer and Philosopher and as Statesman and Diplomatist and 26 learned papers were presented. The meeting closed with what was undoubtedly the largest and most brilliant dinner in the whole history of the Society. The records of this meeting are contained in a sumptuous volume of 320 pages to which were added five volumes containing the Calendar of the Franklin papers then in the possession of the American Philosophical Society.

In reviewing these celebrations one cannot fail to be struck by the fact that the dinners, luncheons and formal ceremonies occupy an increasingly important place in the meetings. In these times of national stress and danger we can more appropriately emphasize the work of the Society and its contributions to useful knowledge without altogether neglecting those other features which make this body a society.

Although our Society was modeled on the Royal Society of London which has as its objective "the improvement of natural knowledge," Franklin gave it a more practical turn by devoting it to "the promotion of useful knowledge." The choice of the earliest members reflects this practical turn, viz:

Thomas Bond, Physician
John Bartram, Botanist
Thomas Godfrey, Mathematician
Samuel Rhoads, Mechanician
Wm. Parsons, Geographer
Phineas Bond, General Natural Philosopher
Thomas Hopkinson, *President* (Statesman and Author)
William Coleman, *Treasurer* (Associate Justice of Supreme Court of Pennsylvania)
Benjamin Franklin, *Secretary* (Printer, Philosopher, Statesman, Diplomat).

The earlier work of the Society as reported in the minutes of meetings was more practical than theoretical. In 1769 the reorganized Society was divided into six committees, as follows:

1. Geography, Mathematics, Natural Philosophy and Astronomy
2. Medicine and Anatomy
3. Natural History and Chemistry
4. Trade and Commerce
5. Mechanics and Architecture
6. Husbandry and American Improvements

Although Provost Wm. Smith and Bishop White were early officers, neither arts, letters nor theology were formally recognized in these six committees, but in 1815 a seventh committee of History, Moral Science and General Literature was added and thereafter membership of the Society was drawn from all fields of learning. In a period of increasing specialization and departmentalization of knowledge the American Philosophical Society is one of the few learned societies that include all knowledge in their realm, and this is recognized by the present classification of members into the Mathematical and Physical, the Geological and Biological, the Social Sciences, and the Humanities. If the scientists of Classes I and II are inclined to extend their borders so as to include the Social Sciences and Humanities, the latter can also extend theirs to include the sciences for all science is human science and in this Society the sciences and the humanities should and do overlap and furnish mutual stimulus and sympathy.

This program should demonstrate the unity of science and learning. Each topic represented is a phase of human history as well as of a specific subject. For various reasons not all the work of this Society could be represented in this program, but it is planned to hold another meeting at a later date dealing with subjects omitted here.

We are all deeply indebted to those scholars who have consented to take part in this program. Many have come from a distance, all have turned aside from important work to prepare papers for this occasion. We expect to publish these papers in our PROCEEDINGS and thus give them a wider distribution among all who are interested in the history of science and learning and its survival and progress here and among all nations.

JAMES LOGAN, A COLONIAL MATHEMATICAL SCHOLAR, AND THE FIRST COPY OF NEWTON'S *PRINCIPIA* TO ARRIVE IN THE COLONY*

FREDERICK E. BRASCH

Chief, Smithsonian Division, Library of Congress

(*Read February 13, 1942, in Symposium on the Early History of Science and Learning in America*)

THE American Philosophical Society, in designating this program "The Early History of Science and Learning in America," has incidentally selected the tercentenary (1942) of the birth of the great Sir Isaac Newton. It can truly be said that no one person has had such a tremendous influence on the scientific and philosophical thought in the world and particularly in colonial America as Newton. However, it is not the function of this paper to recall the various features of Newton's work which have helped to mold scientific thinking in the colonies but merely to call your attention to one of a remarkable group of young scholars who helped to engender and create the Newtonian epoch in the American colonies.

That Philadelphia should have been so far advanced in matter of this character is in keeping with its brilliant colonial life. Benjamin Franklin, David Rittenhouse, Cadwallader Colden, Thomas Jefferson and other advanced thinkers associated with this ancient and honorable body of scholars, reflected the same glory and ideals of that society which Barrow, Newton, Halley, Wren and Boyle helped to create, namely the Royal Society of London.

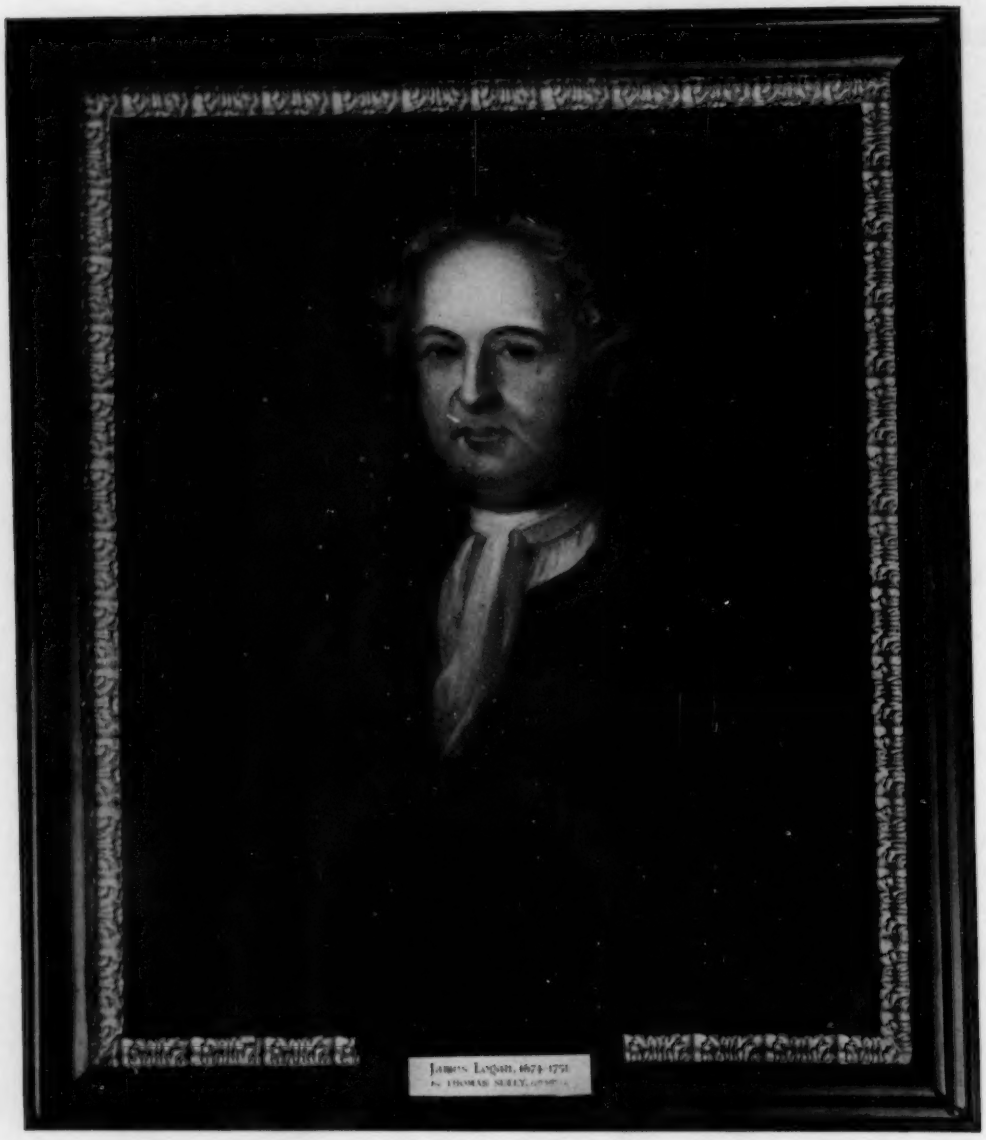
In the northern colony of Massachusetts Bay, we find at this period Cotton Mather, the high-priest of learning at Harvard College, expounding to his hearers the virtues of Newton's life and works while Thomas Brattle furnished valuable observations on comets to Newton and Halley and John Winthrop expounded in systematic manner Newton's laws of gravitation and optics before the students in Harvard. A further Newtonian stimulus was given to the scientific thought in America through the visit to the colony from 1732 to 1734 of Dean Berkeley, the Philosophical critic of Newton.

However, in this program we bring to you something new concerning a relatively unknown disciple of Newton, one who may also be thought of as the first mathematical scholar in the American colonies, James Logan, a brilliant young scholar and schoolmaster, who accompanied William Penn on his second visit to his colony in 1699 as his private secretary.

James Logan was born in Armagh, Ireland, on October 20, 1674, and was educated mostly by his father, who was an M.A. from the University of Edinburgh. Logan Sr. was a clergyman in the established church of England until his conversion to Quakerism in 1671. He later became Master of a Latin School in Armagh where young Logan was born. What influence James' mother had over his life and his education is not recorded, but it is evident that she must have had considerable and effective influence, because of the long absence of the father during the trying times of the family life. She encouraged his self education by purchasing books when funds were not always forthcoming, particularly while residing in Edinburgh. Young Logan, at an early age, showed decidedly scholarly inclinations, having acquired Latin, Greek, and Hebrew as a foundation for a classical education. During a short stay in Edinburgh with his mother, while the father was seeking a schoolmaster's position in Bristol, the boy, James, came upon a copy of Leybourn's *Cursus Mathematicus* (1690) which was then just published. This was the turning point in his career, for he said "I made myself master of that subject without any manner of instructions." The family soon came together again at Bristol, where James also became a schoolmaster. Here he carried out, in private hours, further study of mathematics, astronomy and botany, which habit he fostered all his life. Leybourn's *Cursus Mathematicus*[1] was an unusual compendium of mathematical and astronomical knowledge, especially prepared for the education of the surveyors, navigators, and explorers of that

* Acknowledgment is hereby given to the American Council of Learned Societies and the American Philosophical Society for grants in aid during the years 1932 and 1937 respectively.

[1] Leybourn, William, Philomath. Mathematical Sciences in 9 books. London, MDCXC.



JAMES LOGAN 1674–1751
By Thomas Sully

period. It also gave excellent elementary working principles of the sciences most necessary for the practical use of the student and layman. It was not, however, a treatise for those preparing for Cambridge, Oxford or Edinburgh Universities. Leybourn (1626–1700) himself was not a trained scholar of the colleges. He was, nevertheless, a teacher of mathematics and a surveyor of London and a textbook writer of considerable ability, publishing a large number of books on Algebra, Geometry, Trigonometry, Cosmography, Dialling and Logarithmic tables. This compendium of mathematical sciences consists of nine "books" or chapters comprehending Arithmetic, subdivided into vulgar, decimal, instrumental and algebraical; Geometry, plain [sic] and solid; Cosmography, celestial and terrestrial; Astronomy, theoretical and practical; Navigation, plain and spherical; and Trigonometry, rectilinear and circular; with the description, construction and use of geometrical and nautical instruments. The treatment of algebra is extremely limited and acts as a supplement to geometry. No references are made to quadratic equations and binomial theo-

rems. Curiously the treatment of plane trigonometry occupies the larger aspect of the book. The book on Astronomy in itself is extremely limited and first treats of the problems relating to astrology, also with the various theories concerning the solar system, namely 1, Pythagore-Ptolemai; 2, Plato-Porphyrius; 3, Egyptian system; 4, Systema Philolai and Copernici; 5, Systema Tychonicum; and 6, Systema Tychonicum-Riccioli. This is followed by Tycho's Solar and Lunar Tables and the Rudolphus' Tables calculated both in decimal parts of a circle and in degrees, and a Triangular Canon Logarithmical containing the logarithms of all absolute numbers from unity to 10,000. The tables for the sun, moon and the six known planets are calculated for the meridian of Uraniborg. While this book was published in 1690, there is no mention of Kepler's laws excepting the first, nor anything concerning the law of falling bodies of Galileo. But, above all, it is totally lacking in any reference to Newton's work on gravitation, or reference to the *Principia,* which had already appeared and had been discussed thoroughly for the previous three years by those who were able to understand the new science of the laws of celestial motion. Probably it was too advanced for Leybourn to include in his work.

The year 1699 brought Logan the opportunity for a new life and a wider experience in a strange world. Just what the occasion was that brought Logan to the attention of William Penn does not seem to be recorded, except possibly by the bond of common religious principles. However, Logan was offered a secretaryship to the Founder of the province of Pennsylvania which he readily accepted. On his arrival in the colony, the young secretary soon made his new environment his home, and also found greater possibilities than were at first anticipated, for this unusual task of secretary was of greater scope than that which ordinarily befell a secretary of those days. For Logan, at the early age of twenty-seven, gradually and skillfully assumed the control of the administrative affairs of the colony, and complete management after Penn's second return to England in 1701.

Young Logan married into one of the most prominent Quaker families of Pennsylvania, and settled in "Stenton," near Philadelphia, in 1714. To this place he retired in 1747 from his successful career as counselor and administrator, and devoted the remaining years of his life to scientific and philosophical meditation. For twenty-five years he carried on researches and study in botany and the mathematical sciences. With characteristic foresight, while a teacher and administrator, he gathered together one of the finest as well as the largest collections of books on mathematics and the physical sciences.[2]

This collection virtually ranked first in all the British colonies, in either private homes or college libraries. His correspondence with scientific men at home and abroad was probably only equalled by that of Franklin who was then beginning to assume prominence in the affairs of the United Colonies. His chief scientific correspondence abroad was mostly with Peter Collinson, the noted botanist residing near London, Sir Hans Sloane, President of the Royal Society, Edmund Halley and William Jones, mathematicians and friends of Sir Isaac Newton, and with the Secretaries of the Royal Society of London. The importance of his correspondence with Jones will be noted later, for it is in these letters, namely, Logan to Jones, that much of his mathematical knowledge is revealed. Unfortunately, Jones' replies to Logan do not seem to be extant. However, it is the first series of letters in which is shown his true mathematical genius and a critical mind uncommon in those days.[3]

James Logan was practically, as has been intimated, a self-educated man, not having the advantage of a formal university discipline such as might have been obtained at Cambridge or Oxford. But, like a true scholar which indeed he was, we find that he surrounded himself with the great and fundamental books of the past. Therefore, before analyzing Logan's scientific thoughts and writings, it will be of interest to look into his remarkable library.[4] As an added proof of Logan's self-discipline and methods of study, we find many of his books are annotated, corrections made, and extra sheets inserted containing problems and their solutions. This, indeed, is the trait of a critical and discerning scholar, and it might be added, that such notes are a particular delight to historians and bibliographers. The classes of subjects comprising the larger part of the library of nearly two hundred and fifty volumes of the great classics in the history of science,

[2] Gray, Austin K., *Benjamin Franklin's Library.* New York, 1937. See pages 73–80, Loganian Library.

[3] Rigaud, S. P. *Correspondence of Scientific Men of the 17th Century.* Oxford, 1841, pages 282–341.

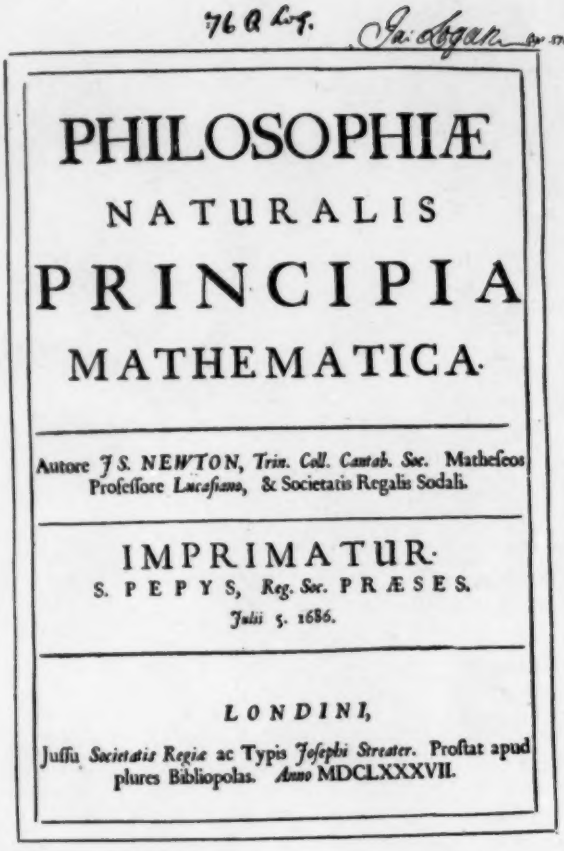
[4] Catalogue of Books belonging to the Loganian Library to which is prefixed a short account of the Institution. Philadelphia, 1837.

include a goodly number of the great mathematical works of the Greeks, namely, Archimedes, 4 treatises; Apollonius, 3 works; Euclid, 9 different treatises; Pappus and Ptolemy, 2 copies each. We find also the works of Diophantus, Petavius, all in the original Latin and Greek, and a number of minor Greek geometers. Of the later period, beginning with the Renaissance and ending with Newton, we find the works of Copernicus, Brahe, Kepler, Galileo, Descartes, Flamsteed, Halley, Barrow, Huyghens, Wallis and many other famous authors on astronomy, mathematics and physics. A full list of these master treatises and comments thereon is being reserved for a complete treatment of Logan's scientific life.

Isolated as Logan was from current scientific thought and progress, his correspondence was the only means of keeping in touch with affairs in Europe. Had he had access to the *Transactions of the Royal Society of London,* his progress might have been different, therefore we find that he made a more intensive use of his books as a consequence.

Since the subject of this paper was announced as two-fold in character, we will now, for a moment, give particular attention to the second subject, namely Newton's *Principia.* Logan has the distinction of being the first person in America to possess a copy of Isaac Newton's *Philosophiae Naturalis Principia Mathematica,* 1st edition, 1687. The date, being in the upper right-hand corner of the title page, is April 1708 (see Fig.). We discuss later the contents of this book; suffice it to say here, it made mathematical history in the colony. We find that Logan also possessed the second edition, 1713, edited by Cotes, and the third edition, 1726, edited by Pemberton, and the first edition of the *Optics,* 1704. In all, his library had the most complete collection of Newton's writings in the colony, approximately 16 different titles, and this far more than of any other author.

Newton's *Principia* enjoys an almost absolute position of fundamental importance in the history of astronomy, mathematics, and analytical mechanics. Its conception was truly the work of a genius and marks the beginning of modern mathematical astronomy. The philosophy of Newton's work was based upon the prevailing theories of gravitational force and the three laws of Kepler, together with Galileo's laws of falling bodies. Newton brought order out of chaos. His contemporaries had speculated on the force that held celestial bodies such as planets and comets in their places, but it was he who proved by calculation that bodies do move in regular defined orbits by reason of the attractive force we now call gravitation.


76 A Log. Ja: Logan

PHILOSOPHIÆ
NATURALIS
PRINCIPIA
MATHEMATICA.

Autore *J S. NEWTON, Trin. Coll. Cantab. Soc.* Matheseos Professore *Lucasiano,* & Societatis Regalis Sodali.

IMPRIMATUR.
S. PEPYS, *Reg. Soc.* PRÆSES.
Julii 5. 1686.

LONDINI,
Jussu *Societatis Regiæ* ac Typis *Josephi Streater.* Prostat apud plures Bibliopolas. *Anno* MDCLXXXVII.

First copy of the "Principia," 1687, to arrive in the colonies. Now in the Loganian Library, Philadelphia. By courtesy of Dr. A. K. Gray, Librarian.

The *Principia* enlarged upon Kepler's three laws of planetary motion and Newton redefined these laws in terms of the central attraction of the sun. The fundamental principles upon which all problems in theoretical and celestial mechanics depend are Newton's three axioms or laws of motion. The *Principia* is an elaborate application of these laws by a synthetical or geometrical method. Today these may be derived by employing the analytical method known as the differential and integral calculus.

The *Principia* is made up of three books[5] and the following shows the contents of each:

[5] Ball, W. W. Rouse. *Essay on Newton's Principia.* London, 1893, pp. 63–65.

The first book is given up to the consideration of the motion of particles or bodies in free space either in known orbits, or under the action of known forces, or under their mutual attraction. In it Newton generalized the law of attraction into a statement that every particle of matter in the universe attracts every other particle with a force which varies directly as the product of their masses and inversely as the square of the distance between them; and he thence deduced the law of attraction for spherical shells of constant density.

The second book treats of motion in a resisting medium, and of hydrodynamics, with special application to waves, tides and acoustics. Newton concluded it by showing that the Cartesian theory of vortices is inconsistent with known facts and with the laws of motion.

The preparation of the third book required a knowledge of numerous astronomical data with most of which Newton must have been previously unacquainted. It was fortunate that he was able to consult so skillful an astronomer as Flamsteed, from whom he obtained much of the necessary information.

It is not the function of this paper to elaborate further or to give an account in detail of the contents of this book beyond that which has just been stated, therefore we will next attempt to show how Logan mastered the mathematical principles demonstrated by the many propositions, problems and theorems which are set forth in elegant mathematical form. Logan's preparation for this task was self-imposed and that he succeeded in it is shown by his many letters, notes, and papers. We will now review the critical comments and corrections found in his copy of the *Principia*. For anyone to attempt to master the principles of celestial mechanics as defined in the Newtonian manner or in the analytical method of today, is indeed a task calling for unusual mathematical skill. Bearing in mind that Logan was not prepared as one would be today and was without mathematical companionship and commentators, it is truly amazing that he accomplished as much as he did. In making these various annotations and comments, he seems to have solved for himself some of the more difficult problems. On the whole, however, he was able to comprehend the complete work of the *Principia*. This is indeed an indication of great mathematical skill.

The first of these difficult problems to be met with is one on the measure of curvature which is to be found in Lemma XI[6] together with Scholium.[7] The first eleven lemmas, or section 1, has to do with the method of primes or ultimate ratios. The last, which is a scholium, has to do with the comparison of the methods of limits and the methods of exhaustions, and method of indivisibles.

Logan had given considerable attention to these lemmas for he realized the importance of them as the foundation of the theory of fluxions or calculus. The geometrical interpretation of this theory is difficult to follow and understand but today it is made much simpler by the algebraic analysis which is ordinarily employed. Space will not permit us to give a demonstration showing by comparison Newton's method and the modern method of the meaning of the prime and ultimate ratios.

We next find Logan studying the determination of centripetal forces which is in the second section of book I. Here he has found proposition IV, theorem IV, with nine corollaries[8] and the scholium, of particular interest. His notes again show that further elucidation was necessary, and contain a critical analysis of all nine corollaries giving examples and proof of each. It is also clear that he proved to his own mind just what Newton meant, and how he arrived at the results. To give one more example, we find in book I, section 6, how the motions of the bodies in given orbits are to be found. Proposition 30, problem 22, is to find at any given assigned time the place of the body moving in a given parabolic trajectory or orbit. This proposition and the one following, namely 31, are analogous to Kepler's harmonic law discovered in 1609. Here, however, Newton definitely expressed this proposition in terms of force. The importance of this section on the determination of the motion in given conics or the determination of the motion of bodies in an ellipse or parabola is fundamental in celestial mechanics. This was also noted by Logan in his subsequent studies.

In these three cases just recited his work is represented by writing out his own geometrical equations, his reasoning and the solution of the problem. The thoroughness of Logan's attention to all that he did not understand is manifestly a trait of a master mind.

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[6] Lemma—a proposition assumed or demonstrated which is subsidiary to some other.

[7] Scholium—a note added by the author illustrating or further developing some point treated in the text.

[8] Corollary—a proposition appended to another which has been demonstrated and following immediately from it without new proof, hence an immediate inference, deduction, consequent.

The one complete manuscript left by Logan and written in Latin has to do with a partial review and critical analysis of Halley's Astronomical Tables. He secured these tables in unbound form while in London in 1723 and says of them "they were lying [in] the shop of the book-dealer William Innys, at whose expense they had been set up in type, and he sold them in sheets to me privately for a very moderate price." These sheets were printed in 1719 but were not bound or published until 1752, ten years after Halley's death. These tables have to do with determining the coördinance or places of the sun and moon, planets and certain stars, particularly for the use of mariners. This manuscript, entitled *"Calculus motus Lunæ ex his Tabulis Halleianis per J. L.,"* was probably written sometime shortly after Halley's death. The most interesting part concerning this essay besides its critical review of the various tables is that it reveals further excursions into Newton's *Principia*. So thorough was Logan's knowledge of the contents of the *Principia* that he is constantly referring to some proposition or theorem as a basis for argument. Logan takes up the question of the moon's eccentricity and also of its apogee and nodes throughout the medial anomaly of the sun. He analyzes the moon's motion in its orbit in great detail particularly in reference to the data furnished by Halley's table. This work is shown by example together with diagram. In this review Logan does not hesitate to point out errors in construction or computation on the part of Halley. Of the remaining tables, Logan said "they are helpful for computation of eclipses and hardly require any elucidation." But it is in these tables that Logan discovered an error relating to a predicted total eclipse of the sun on May 11, 1724, which was not completely total at the particular place, Windsor near London, as Halley had predicted.

The study of the moon's motion has been one of tremendous importance in astronomical research and one of the most difficult as a mathematical problem. This is due to the many factors involved together with unknown elements in these factors. Newton realized, as a dynamical problem of the first order, the importance of the moon's motion in relation to the sun and the earth. Some of the most brilliant mathematical scholars in America have given a lifetime to evaluating these factors, notably Simon Newcomb, George W. Hill and the late E. W. Brown. Logan was the first in the colonies to appreciate this problem and the difficulties involved for he attempted to analyze the moon's orbital motion according to the propositions laid down by Newton. However, the problem attacked in this manuscript involves no great difficulty in so far as determining the anomaly, variation and latitude of the moon's position by trigonometrical functions. A further analysis of these tables would involve greater use of technical phrases.

Logan's further contribution to astronomy and mathematical studies is revealed in a series of letters written to William Jones.[9] Jones was a mathematical scholar of considerable importance, a friend of Newton as well as a prominent Fellow of the Royal Society. Undoubtedly this contact with Jones was arranged by Peter Collinson, with whom he had been in correspondence in matters pertaining to botany. Collinson was another Benjamin Franklin in that he was constantly interested in the works of others and realized that community of interest was the way of scientific and personal progress. Logan had great need for an outlet for his expression in mathematical studies and while he constantly sought the aid and encouragement of some of his local contemporaries, his greater aim was to converse with professional scholars. We have evidence of this in a series of letters written by Logan to Jones but those from Jones to Logan are not to be found. The first letter to Jones was dated Philadelphia, November 8, 1732. It has to do with acknowledging a gift of three volumes of Motte's first English translation of the *Principia* published in 1729. Here again Logan displays a scientific insight by taking up a question of misstatement as found in the Appendix by John Machin on the moon's motion, supposed to be an improvement on Newton's theory. In this letter Logan makes the statement: "I am willing to impute it to my own incapacity that I can by no means comprehend his law of motion where a body is deflected by two forces tending constantly to two fixed points vis. that it will describe by lines drawn from the two fixed points equal *solids* and equal time about the line joining the said fixed points." Comparing the various editions we find that the word *solid* is a misprint and subsequent editions of Motte's translations have omitted Machin's Appendix entirely. The confusion in this statement in Logan's mind apparently is found also in the fact that the law of areas holds exactly true where two bodies are concerned such as the sun and earth. But when

[9] See Rigaud's *Correspondence.*

a third body is introduced, for example the moon, the problem becomes more complex due to other inherent elements, one being perturbation. It is not a little surprising that Logan did not recognize this fact. Newton had already traced the various disturbing effects produced by a third body upon the other two but no complete analysis had been made until 1745 by Euler, the Swiss mathematician, who was followed by Clairaut and Delambert of France. The problem of perturbation is one of the most fascinating in the determination of the motion of planets in their orbits and constitutes a test for the best mathematical minds. What Jones' reply was to Logan concerning this perplexity is not known, except in a letter of acknowledgment to Jones a few years later where he thanks him and apologizes for his troublesome inquiries.

It was but natural now to find that Logan made inquiries into the field of pure mathematics and particularly concerning the problems of infinite series and fluxions. This was the outcome of his study on curvature already mentioned. In a letter to Jones he appeals for aid and probable methods for further enlightenment. As so often happened in those days, exchange of letters between these two scholars was of long and slow duration. In the meantime Logan found his way through to a solution. Since the methods of infinite series and fluxions was lately discovered, it was therefore uppermost in the minds of the scholars at home. Newton in the year 1665 had developed the fluxion theory now known as the calculus while Leibnitz invented convenient symbols and fundamental rules for differentiation and integration. The Newtonian scheme is called fluxions of different orders because intended to express the rate of change of quantities by the change of others, and Leibnitz' system is called infinitesimal, being conceived to express cumulative addition of indefinitely small quantities. Taking the fluxions, or the differences from the quantity generated by the change or by the addition, is called the direct method; taking the quantity generated from the fluxions or finding the sum of all the differences, is called the indirect method.

The Newtonian theory calls the direct method that of finding fluxions, the indirect method that of finding fluents. The Leibnitz method calls the former differentiation, or finding the differential, the latter integration, or finding integrals. The two systems do not differ except in the manner of expressing the operations and in the symbols used; the principles and results are the same. A further extension of this system and operations by higher powers and different equations would be inappropriate for discussion in this paper. Suffice it to say, Logan became a master of the Newtonian method for studying and solving elliptical equations. The problem that he was keenly interested in was that which had to do with the length of an elliptical curve. This has the characteristic properties of problems in orbital motion. Here again he had trouble in getting a perfect solution for the part of the curvature he wanted because he found no converging series. Surprised at this, he consulted his friend, Thomas Godfrey, and he, too, found no converging series to aid them. Logan had the great ability, when in difficulty, to strike out canons, as he called them, of his own invention. This was indeed the mark of a true mathematician. However, in writing to Jones in June, 1736, he again was apprehensive of possible errors on the part of Newton, but a prompt reply set Logan right. He continued his studies of infinite series by the methods of fluxion more in the spirit of the joy of doing the work than of trying to create or discover something new. Up to the present those canons he struck out are not known to mathematicians, which is unfortunate as these in themselves may have some foundation for further investigation.

In May, 1738, Logan addressed a letter to Jones on the subject of quadratic equations. From a paper published in the *Transactions of the Royal Society of London,* Vol. 18, 1694, he notes Halley's rules for resolving affected equations. Logan was apparently not satisfied by these rules and proceeded to clarify them in order to make them more intelligible. This letter shows how Logan adopted a more complete analysis of his own by setting up his own equation and finding a solution. True, his problem falls within the course of advanced algebra, and thereby shows progressive efforts, originality and invention, in a subject not known to the advanced mathematical mind of that period.

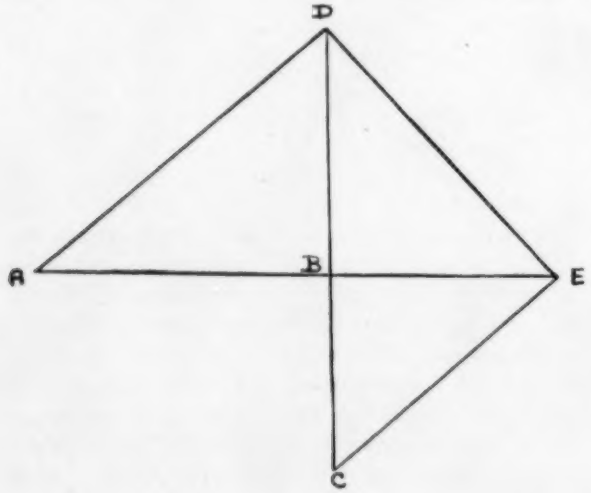
A further indication of Logan's ingenuity in the field of mathematical problems is shown when he attempts to solve one of the three classical problems in pure geometry. In his wide reading, in both Latin and Greek authors, he came upon the problem of the duplication of the cube. In this particular case he seems to have received some encouragement from Isaac Barrow in his Compendium edition of Archimedes (p. 31) in which

it is stated, "This is a real problem, requiring in fact for its solution that two mean proportionals should be found, which ordinary geometry, using only the compass cannot provide. It can be accomplished by means of conic sections and in several other ways that I do not here mention."

The problem as proposed to Jones in his letter of March 31, 1738, is as follows: "How hard the ancients struggled with the solving of the Delian problem (as it used to be called) or the duplication of the cube, *i.e.*, with discovering two mean proportionals is quite well known from Eutocius, Eratosthenes, Pappus and others. For to this single problem is said to be due the discovery of the conchoid and the cissoid; nay, more, it is believed that from this the first steps in conics took their origin. Various solutions of it have been found, but they are almost all intricate and laborious as is abundantly evident in the authors just named. What may have been offered by more recent authors that is better and more susceptible of geometrical operation, I do not know. A method of solving it has, however, occurred to me, which, if it has not been noticed by others, will perhaps not seem unworthy of being published.

"That it is a serious problem and cannot be solved by simple geometry or by the straight line and the circle, is admitted by geometricians. And if, the following method, by the mere use of straight lines is found as easy as the solution of (I should almost say) the easiest of the Euclidean problems, there would hardly seem to be any reason why it should not be regarded as purely geometrical. This, then, is it.

"Two given lines AB and BC form a right angle at B (see Fig.) and are extended toward E and D; two parallels are next drawn through the given points A and C, cutting other lines at D and E, in such a way that the line DE connecting these parallels is perpendicular to each, then that which is desired is accomplished, as is evident at a mere glance. For the triangles CBE, EBD and DBA are similar on account of the right angles, and therefore as the given line BC is to BE, so is BE to BD and so is BD to the given line AB, and between the two given lines are two means, BE and BD. Q.E.D.



"It may perhaps be objected that the operation is not purely geometrical for the points E and D are not directly given, but are merely discovered by experiment. But since the square or angle are instruments equally serviceable to simple geometry as the compass, the result, it will be granted, is very easily accomplished by this means; and very promptly, too, if that scale is applied which is usually kept in cases of geometrical instruments for drawing parallels. For if the one side of it is applied to the point A, and the other to C, and its two angles are applied to the straight lines BD and BE equally, which is very easy to do, then these points are very quickly determined. And by this method two geometrical means are given, no more laboriously than a single [point] of the teaching of Euclid."

Logan apparently had read extensively into the history of these problems and particularly on the duplication of the cube and after considerable meditation on a new method of solving these problems, he ventured to propose it, but in a following letter to Jones, he apologizes for his haste for he said he discovered that it was not new, that is for its solution, that two mean proportionals should be found. The purpose of calling attention to this letter of Logan's is that it is interesting in revealing his power of originality which has already been pointed out in his studies of algebraic equations and problems of fluxions.

At this time James Logan was confined to his crutches and had much leisure to carry on studies of this sort. During this time he also carried on his botanical research and planned a library for the benefit of future scholars.

Let us look into his studies in optics concerning the laws of reflection and refraction. This subject brings us to two vital phases of his life, namely his contact and acquaintance with Thomas Godfrey, the colonial inventor of the reflecting quadrant, as well as his relation to Edmund Halley. Concerning Godfrey, Logan speaks of him "as a

young man of excellent natural genius for these studies who, under the greatest disadvantages of education and circumstances, has made himself a very good master of Newtonian philosophy." It is with true scholarly spirit that Logan interested himself in Godfrey, and his invention of the reflecting quadrant. It was the purpose of Logan to undertake the study of the laws of reflection and refraction in order to inform the Royal Society of the excellent merits of this instrument. It so happened that the same invention was thought of by an Englishman named Hadley. Here was the beginning of a long period of dispute as to the first date of this invention, for both England and America laid claim to it. The following dates and facts set the question at rest.

Hadley completed one of his reflecting quadrants in the summer of 1730. In the following May (1731) he communicated an account of the instrument to the Royal Society. Godfrey invented a similar instrument in October, 1730, and in 1732 he and Logan both wrote to Halley to give an account of what had been done in Philadelphia. Halley was not then Secretary of the Royal Society but owing to his prominence and knowledge of the sea, he was communicated with probably on the advice of Jones or Collinson. Resulting from some unpleasant reaction over the priority of this invention, there seems to have developed an unfriendly attitude which may have accounted for an oversight of recognition that Logan should have had. With characteristic spirit, Logan continued to master the principles of optics and of light and to some extent experimented with the reflecting telescope. He also studied the phenomena of aberration. In these studies he is constantly referring to authorities and source books but he gives particular attention to Newton's optics of which he had four editions.

Logan published four papers in the *Transactions of the Royal Society*[10] from 1735 to 1738, although he sent a number of others which were not published. For one who was constantly writing and in touch with the leaders of science in England, it is indeed surprising that more encouragement was not given him. He was gifted to carry on extended research in pure mathematics or in celestial mechanics but apparently did not choose to exert himself to complete any one study in which he was interested. It does seem, however, that his was a self-appointed task to search and to criticize and when this was carried out to its logical conclusion, Logan's work was fully equal to that of many brilliant scholars in other parts of the colony. Probably the only reason that he did not choose to publish more was because of his modesty which made him refer to his studies as "only self-amusement." Was it this modesty that prevented him from seeking an election to the Royal Society of London? Recognition by election as Fellow of the Society was a mark of great distinction in the colony.

Possibly a further reason for Logan's indifference concerning this election, was his slight quarrel with Halley regarding Godfrey's invention of the reflecting quadrant together with several critical comments on Halley's astronomical tables and particularly those relating to a solar eclipse near London. This may have been the real stumbling block in view of the fact that Halley was an active and prominent member of the Royal Society.

At this time the American Philosophical Society had not yet been established. Godfrey was a member of the Junto, the germ of this organization, and he was one of the nine original members of the American Philosophical Society, named by Franklin in 1743. Logan, even though he was a friend of Franklin, was not a member of the Junto, the American Philosophical Society or the American Society and he died before the union of these societies in 1769.

Logan had the great distinction and honor of being present at a meeting of the Royal Society of London on the occasion of the presenting of a report of a total solar eclipse which occurred on May 11, 1724, and had been seen near Windsor 20 miles west of London. Newton was then President of the Royal Society and was in the chair to hear this report. It is known that Newton enjoyed meeting strangers from foreign lands since he never travelled himself, so surely Logan would have attracted his attention for he had learned to have high regard for the scholars and observers of the colony. This was shown in Newton's statement concerning comet observations which were made by Arthur Storr of Maryland and Thomas Brattle of Harvard as well as

[10] "An account of Mr. Thomas Godfrey's improvement of Davis's quadrant, transferred to the mariner's-bow," *Philos. Trans.* **38**: 441–450, 1735. "Some experiments concerning the impregnation of the seeds of plants," *Philos. Trans.* **39**: 192–195, 1737. "Concerning the crooked and angular appearance of the streaks, or darts of light'ning in thunder-storms," *Philos. Trans.* **39**: 240, 1737. "Some thoughts concerning the sun and moon, when near the horizon, appearing larger than when near the zenith," *Philos. Trans.* **39**: 404–405, 1738.

other contributions of the colonials published in the *Transactions*. From this we can probably assume that Logan and Newton came together and conversed. Dummer, the Yale College Agent, met Newton in 1714 and it is also recorded that Franklin tried on several occasions to meet him. In the series of letters quoted and from manuscripts left by Logan, his attachment to and admiration for Newton are very evident. He recognized in Newton the very best the Quaker mind adores in man. Newton was to Logan both human and heroic in mind, almost infallible in revealing the solutions of the problems that nature proposes.

In the colonies, Logan's influence was always felt by those who sought his aid, whether in the form of actual assistance in mathematical problems, or in collecting specimens unknown to botanical science or in the loaning of books to deserving students. "Logan was a man of radio-active energy of mind. He could inspire young men with his own intellectual enthusiasm."[11]

In 1751, at the age of 77, Logan passed away. So, after these many years, belated as it may seem, we wish to pay him tribute, for he was our first mathematical scholar and having brought the first copy of the first edition of Newton's *Principia* to the colony, he was able to interpret it with skill and understanding. With the keenest of judgment, he assembled and established the first and finest collection of mathematical and astronomical literature in the colonies which, fortunately, is still in active service in the Loganian Library incorporated with the Library Company of Philadelphia in 1792.

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[11] Gray, A. K., *Benjamin Franklin's Library*, page 75.

ASTRONOMY DURING THE EARLY YEARS OF THE AMERICAN PHILOSOPHICAL SOCIETY

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(*Read February 13, 1942, in Symposium on the Early History of Science and Learning in America*)

"It was not without being sensible how very unequal I am to the undertaking that I first consented to comply with the request of several gentlemen for whom I have the highest esteem, and to solicit your attention to a subject which an able hand might indeed render both entertaining and instructive. I mean Astronomy." These words are not my own but rather they were spoken by David Rittenhouse in an "Oration" delivered February 24, 1775, before the American Philosophical Society. According to the minutes of the Society, the oration was delivered in the College "to a crowded audience, consisting of his Honor the Governor of the Province, the Assembly, and a great number of gentlemen of the first distinction, besides the members of the Society." Moreover, the oration was dedicated "To the delegates of the Thirteen Colonies, assembled in Congress at Philadelphia, to whom the future liberties, and consequently the virtue, improvement in science and happiness in America are intrusted."

Again to quote the words of Rittenhouse, "Astronomy, like the Christian religion, if you will allow me the comparison, has a much greater influence on our knowledge in general, and perhaps on our manners too, than is commonly imagined. Though but few men are its particular votaries, yet the light it affords is universally diffused among us; and it is difficult for us to divest ourselves of its influence, ..."

You are all of you very familiar with the fact that the American Philosophical Society was founded in the year 1743. A valued letter from Benjamin Franklin kept in the Society's library gives the names of the nine original members, with himself as secretary. (In 1743, Franklin, the 15th of 17 children, was 37 years old.)

At a meeting held in the State House on February 2, 1768, we read in the minutes, "A Society having subsisted for some time in this City under the name of the American Society held at Philadelphia for promoting and propagating useful Knowledge, whose Views have been nearly the same with those which are published of the American Philosophical Society, and it being judged that the Ends proposed by both could be carried on with more advantage to the Public if a Union could take place between them, it is proposed that such a Union may take place." Accordingly during the year 1768, the Society took on a new lease of life and more than 100 members were elected.

Early in the following year, on Monday, January 2, 1769, the foremost citizen of Philadelphia, Benjamin Franklin, was elected president of the Society, an office he continued to hold until his death in April, 1790. David Rittenhouse succeeded Franklin as president and he continued in office until his death in June 1796.

As your speaker today has lived for more than 25 years in Virginia on Mount Jefferson in sight of Monticello, four miles distant, I hope you will pardon me for quoting from the letter of the four secretaries of the Society in advising Thomas Jefferson of his election as the Society's president. Under date of January 7, 1797, the letter reads, "We have the pleasure of informing you, that at the annual Election of Officers of the American Philosophical Society for promoting useful knowledge, held at Philadelphia on the 6th inst., you were chosen President of that respectable Institution."

To this Jefferson replied, "I have duly received your favour of the 7th inst., informing me, that the American Philosophical Society have been pleased to name me their President. The suffrage of a body which comprehends whatever the American World has of distinction in Philosophy & Science in general, is the most flattering incident of my life, and that to which I am the most sensible. . . . Permit me to avail myself of this opportunity of expressing the sincere Grief I feel for the loss of our beloved Rittenhouse. Genius, Science, modesty, purity of morals, simplicity of manners, marked him one of Nature's best samples of the Perfection she can cover under the human form. Surely, no Society till ours, within the same compass of time, ever had to deplore the loss

of two such members as Franklin & Rittenhouse. Franklin, our Patriarch, whom Philosophy and Philanthropy announced the first of men, and whose name will be like a star of the first magnitude in the firmament of heaven, when the memory of those who have surrounded & obscured him, will be lost in the Abyss of time."

Jefferson continued as president of the Society during the eight years he was President of the United States. Late in the year 1808, when he was planning soon to move from Washington back to his home at Monticello, Jefferson tendered his resignation as president of the Society. This resignation, however, was not accepted and he continued in office until January 6, 1815. Franklin, Rittenhouse and Jefferson between them had been presidents of the American Philosophical Society from 1769 to 1815, a space of almost half a century.

The real underlying cause for the awakening to life of the Society in the year 1768 is not hard to find. It was actually and literally a heaven-sent event, namely, a Transit of Venus that was most successfully observed in and near the city of Philadelphia. The genius who organized and who was chiefly responsible for carrying the plans through to perfection was David Rittenhouse. Generous praise has been bestowed upon him for the observations that gave a much improved value of the astronomical unit of distance, that of the earth to the sun.

The older members of the Society have heard time and time again about Rittenhouse. Without desiring to withdraw one iota of praise justly due him, I would like to call your attention to members of the Society whose names are now almost forgotten, men who not only took a prominent part in the Transit of Venus observations but who had keen astronomical interests and made valuable observations. I have in mind two provosts of the institution now known as the University of Pennsylvania, Rev. William Smith, provost of the College of Philadelphia, and his successor in office John Ewing, provost of the University of Pennsylvania.

Today I wish to call your attention briefly to some of the high spots in the life of Rittenhouse and at the same time to suggest particularly to the younger members that they go upstairs to the members' room and see the astronomical clock and the transit telescope, both made and used in his observations of the Transit of Venus on June 3, 1769. Upstairs you will find a Dollond refractor also used for the Transit and likewise a theodolite that formerly belonged to William Penn and which was used in laying out the City of Philadelphia.

Rittenhouse was born on April 8, 1732, in a small stone house that now stands on the main drive through Fairmount Park. Even as a boy he showed extraordinary mathematical and mechanical ability and as he grew older he was always very resourceful in supplying his own needs. He acquired great proficiency in observational, practical and theoretical astronomy and in instrument making. The construction of his first telescope dates from 1756. His first observations as an astronomer were in 1763–64 when he carried out a survey for William Penn to settle the boundary dispute with Lord Baltimore. This boundary, between the present states of Pennsylvania and Maryland, was done with such high accuracy that it became part of the Mason and Dixon line.

His early fame, however, rested primarily on the construction of his celebrated orrery of 1767, acquired by a college founded in New Jersey in 1746, and now known as Princeton University. This first orrery was lost but a second one is now in the proud possession of the University of Pennsylvania. For each of these instruments Rittenhouse received £300.

It is most interesting to read over the minutes of the Society after its reorganization in 1768. At the meeting of March 8 held at the State House, "a fine of one shilling for non-attendance of members at meetings was ordered as a by-law of the Society. Six organic committees were then constituted by distributing the members present among the committees." At this early period in the life of the first learned society in the western hemisphere, rewards for attendance and penalties for non-attendance were put into effect. The minutes of succeeding meetings of the Society furnish no information, however, regarding the amount the treasury's balance was enhanced by the one shilling fines.

At the very next meeting on March 22, 1768, we read in the minutes, "Mr. David Rittenhouse communicated to the society by the hands of Dr. Smith a Description of his new invented Orrery, which is referred to the committee of Nat. Philosophy, Astronomy, etc." At the following meeting, April 19, 1768, the committee on Rittenhouse's orrery reported that "they have the greatest Reason to expect from his known Abilities," that the orrery "will do honor to himself & to this

Province, the place of his Nativity; and the Committee beg Leave to recommend it to the Society to order his description to be published; not doubting but it will give Pleasure to Persons of a curious and Philosophical Turn." Hence the first paper ordered to be published by action of the Society was one on astronomy, which then as now one and three-quarter centuries later had a strong popular appeal.

The description which appears as the first article in the first volume of the Society's *Transactions* states that the orrery represents the motions of the sun, moon and planets, and therefore illustrates solar and lunar eclipses and other phenomena "for a period of 5000 years, either forward or backward." For showing the motions of the heavenly bodies it was the forerunner of the planetarium. Jefferson spoke of the orrery in these words: Rittenhouse "has indeed not made a world; but he has by imitation approached nearer its maker than any man who has lived from the creation to this day."

This same meeting of the Society on April 19, 1768, was a memorable one as seen from the following "proposal" presented by John Ewing, one of the secretaries. "After having gone through the calculation and projection of the next Transit of Venus, on the 3rd of June, 1769, I find that the beginning and a great part of it will be visible at Philadelphia, if the weather should be favourable. As much depends upon this important phenomenon, and as astronomy may be brought to a much greater perfection than it has yet arrived at by a multiplicity of accurate observations made of this transit in different parts of the world and compared together, I would humbly propose to this Society that effectual provision be made for taking of the observations in this city. This is the more necessary, as such another opportunity will not be presented for more than a century to come."

The minutes of the meeting on June 21, 1768, read, "Dr. Smith laid before the Society a Projection of the ensuing Transit of Venus over the Disc of the Sun, which was communicated to him by Mr. D. Rittenhouse of Norrington." Also at this same meeting John Ewing communicated quite a lengthy mathematical discussion regarding the Transit and concluded in the following words: "It would be proper that two or more sets of observers be appointed to view the transit in this city, to guard against a fatal accident of losing the sun out of the field of the telescope in the critical and important moment, which I find happened to a good astronomer in the East Indies at the time of the last Transit. It is very difficult to preserve a celestial object in the field of a telescope that magnifies considerably.

"The expence of time and money to make these observations with that accuracy that would do honour to our infant Society must be considerable; but it is hoped that an opportunity will not be neglected on this account, which for its importance to the interests of astronomy and navigation, has justly drawn the attention of every civilized nation in the world."

The Society may well be proud of John Ewing. Later when he was provost of the University of Pennsylvania and likewise professor of natural philosophy, it was said of him that "in the absence of any other professor, the Provost could take his place, at an hour's warning, and conduct the instruction appropriate to that Professorship with more skill, taste and advantage than the incumbent of the chair himself."

The condition at that time of the country surrounding the City of Brotherly Love may be surmised by reading from the minutes of the Society that an inquiry was made "to know whether the Indians would allow proper Persons to pass through their Country in order to make the Transit of Venus observations." Perhaps the lives of the members of the expeditions sent out from this city were in no greater danger than were the lives of eclipse observers in the year 1900, for we have it on good authority that before crossing the Atlantic Ocean to witness the eclipse, the head of one of the British expeditions appealed to the United States government to protect the lives of the party from the wild natives (sic) of North Carolina. I might add that on arriving in New York the fears were effectively dispelled when the party found themselves aboard a luxurious Pennsylvania train and discovered that they themselves, their baggage and their instruments were routed through to destination—and entirely free of charge. Of course, this was in the good old days now gone forever.

For the great event, Rittenhouse had a clock which he himself had made, a transit telescope also constructed by his own hands and now considered the first telescope made in America, and a refracting telescope. These instruments are now in the members' room upstairs.

For the expenses incurred in observing the Transit of Venus, the Society paid more than £100 from its treasury. Thus we find that Dr. Conklin,

the Chairman of our Research Committee, has long established precedent for paying out Society's funds to aid research.

For carrying on the observations in Philadelphia, the minutes read, "The Honorable House of Representatives had generously granted the privilege of erecting the Observatory in the State House Yard, and voted £100 for erection and observation to be paid to the Society Treasurer." Thus we see that the first observatory in the United States to be created at public expense dates from the year 1768.

In view of the fact that the first volume of the *Transactions* discusses in great detail the observations of the Transit, I need add but little here. Beautifully clear skies greeted the observers at each of the three sites in or near Philadelphia, at the State House Square, at the Rittenhouse farm at Norriton and at Cape Henlopen. Throughout western Europe, on the contrary, the astronomers viewed the Transit through clouds, and moreover the low altitude of the sun made observations more difficult and consequently less accurate.

One quotation from *Transactions,* Vol. I, Appendix page 56, might be repeated. It is from the pen of Dr. William Smith, and reads, "And here, as Dr. Halley expresses it, 'Since Venus, like her sex, is exceeding *coy,* and deigns but in certain ages, to come to the eyes of men, divested of her borrowed dress'; an American cannot help lamenting for his Brother-Astronomers in Europe—men of fame and great abilities—that they were condemned, amid horizontal vapors only to a transient glimpse of this rare phenomenon, and that they could not have shared with us some part, at least, of that *luxury of gazing,* which we enjoyed here."

The accuracy of the observations by Rittenhouse, who was assisted by William Smith and John Lukens, has been attested by Nevil Maskelyne, astronomer royal of England at the time, and by Simon Newcomb. The latter [1] states that "his observations of the celebrated transit of Venus in 1769 have every appearance of being among the best that were made." Combination with Greenwich results by a method improved by Rittenhouse gave 8″.805 for the solar parallax, in remarkable agreement with the value 8″.803, which has been the accepted value until a few months ago when the present astronomer royal of England, H. Spencer Jones, gave the refined value of 8″.790, coming from observations made the world over on the planet Eros.

In 1770, Rittenhouse moved to Philadelphia. His observations included transits of Mercury, solar and lunar eclipses, variable stars, the new planet Uranus (discovered by Herschel in 1781) and comets, including one discovered by himself in 1793. His work in astronomy was many-sided. To adjust instruments in the meridian, in 1785 he invented the collimating telescope, since then universally used. The same year he introduced spider threads into his telescope. In 1786 he made a plane transmission grating, thus anticipating Fraunhofer's grating by about 30 years. Newton had previously used "scratches made in polished plates of glass," but Rittenhouse measured grating intervals and deviations of several orders of spectra. He was frequently engaged in boundary surveys and commissions involving Pennsylvania, Delaware, Maryland, Virginia, New York, New Jersey and Massachusetts. Outside of astronomy, he served on the commission to organize the United States bank and George Washington appointed him the first director of the Mint, on April 14, 1792, thus exemplifying the fact that a resourceful practical astronomer is equipped to tackle almost any job. At the University of Pennsylvania he was professor of astronomy and later trustee and vice-provost. In the American Philosophical Society he served as curator, librarian, secretary, vice-president and president. The highest of the many honors that came to him was his election, when president of our Society, as a foreign member of the Royal Society. The first American to be so honored after the Revolution was James Bowdoin, governor of Massachusetts and president of the American Academy of Arts and Sciences.

The following interesting memorandum in the handwriting of Dr. William Smith is found in a bound copy of Vol. I of the *Transactions* that once was part of the library of the provost of the College of Philadelphia. "Astronomical observations, 1776: This year exhibiting little else but scenes of confusion and distress amidst the calamities of an unhappy war, scarce any attention was paid by the members of the Philosophical Society to astronomical or any other literary subjects. It was agreed however by Mr. Rittenhouse, Mr. Lukens and myself, to look whether *Mercury* would touch the Sun's disk the 2d Nov. this year, as a very small difference of latitude from what the Tables give would have carried the planet

[1] *Dictionary of American Biography,* **15**: 630.

clear of the Sun, but from our observation of this Transit of this planet 1769, we had reason to expect it would pass further on the Sun than Halley's Catalogue gives it."

As an observer of total eclipses of the sun in many quarters of the globe, I wish to direct your attention to the fact that eight days only after the British had evacuated Philadelphia, "The first eclipse [2] of the sun to be carefully observed in the British colonies of America was that of June 24, 1778, which was watched by the astronomer, David Rittenhouse." Although this eclipse was total, no mention was made by the Philadelphia observers of the gorgeous beauty of the corona readily visible without the aid of a telescope. To us at the present day, one of the strangest portions of the history of astronomy up to the middle of the nineteenth century is the dearth of observations of the corona itself and of the prominences readily visible to the unaided eye at the time of a total eclipse of the sun.

A partial explanation of the failure to describe the corona is to be found in the copy of Vol. I of the *Transactions,* before alluded to. The following memorandum appears: "Eclipse of June 24, 1778: observed by Dr. Rittenhouse, John Lukens, Owen Biddle and Wm. Smith at the College of Philada. The morning being very cloudy, the beginning of the eclipse was not seen. . . . 11h 6m 57s o'clock, End of eclipse distinctly seen, the sun having shone clearly for several minutes; the clouds now wholly disappearing and the remainder of the day continuing clear."

The good luck with the weather that greeted the Philadelphia observers at the Transit of Venus could not be expected to follow them always. The arch enemy of all astronomical work is clouds and sometimes more clouds, which are all the more of a calamity on an expedition.

In the midst of the Revolutionary War, in 1778, life in Philadelphia evidently went on about the same whether the British soldiers were present in the city or not. That was ages before we had heard of the word "schrecklichkeit," or of "the scorched earth" policy.

The next total solar eclipse visible in the United States was only two years later, on October 27, 1780. It was not total in Philadelphia but was total in New England. This eclipse is memorable for two distinct features. It was the occasion of the first American eclipse expedition, under Professor Williams of Harvard College. I think you will be interested in hearing some of the details [3] published in *Memoirs American Academy of Arts and Sciences,* 1, 86 (1783). The account by Williams reads, "Though involved in all the calamities and distresses of a severe war, the government discovered all the attention and readiness to promote the cause of science, which could have been expected in the most peaceable and prosperous times; and passed a resolve, directing the Board of War to fit out the Lincoln galley to convey me to Penobscot, or any other port at the eastward, with such assistants as I should judge necessary." When the great day arrived, the Harvard party found itself outside the path of totality. However, an observation of great value was made as you may learn from the following. "The sun's limb became so small as to appear like a circular thread or rather like a very fine horn. Both the ends lost their acuteness and seemed to break off in the form of small drops or stars some of which were round and others of an oblong figure. They would separate for a small distance, some would appear to run together again and then diminish until the whole disappeared." Here is a clear description of the eclipse phenomenon known to all of you as Baily's beads, as the result of observation by Francis Baily, a London stockbroker, at the eclipse more than a half-century later, in 1836.

Another total eclipse of the sun, that of June 16, 1806, was visible in Massachusetts but not in Philadelphia. This might be regarded as an instance of undue partiality to the rival scientific society in Boston, the American Academy of Arts and Sciences. However, I have been pleased to find that a member of the American Philosophical Society, Andrew Ellicott by name, also described Baily's beads thirty years before the 1836 eclipse. The description I have never before seen published nor referred to. I found it in the *Manuscript Communications,* Vol. 2, page 57, in a letter from Andrew Ellicott, part of which reads, "Those detached luminous points of the sun's limb, seemed to retain their brilliancy till the instant of their disappearance, which it would appear should not be the case, if the moon was surrounded with an atmosphere—those points particularly, which are formed by depression in the moon's limb would have had their splendour somewhat diminished by the density of the atmosphere, if one existed,—but nothing of the kind was observed."

[2] Mitchell, S. A., *Eclipses of the Sun,* 128 (1923).

[3] Ibid. 129.

PUBLIC LIBRARY

In the same collection of manuscripts in the Society's library, and four pages after Ellicott's letter, I found another letter regarding the 1806 eclipse, this time from Simeon De Witt of Albany, N. Y., the contents of which pleased me mightily. The letter reads: "With this I send you for the American Philosophical Society a painting intended to represent the central Eclipse of the Sun on the 16th of June last. It is executed by Mr. Ezra Ames, an eminent portrait painter of this place and gives I believe as true a representation of that grand and beautiful phenomenon as can be artificially expressed. The Edge of the moon was strongly illuminated and had the brilliancy of polished silver. No common colors could express this; I therefore directed it to be attempted as you will see by a raised silver rim which in a proper light produces tolerably well the intended effect.

"As no verbal description can give anything like a true idea of this sublime spectacle with which man is so rarely gratified, I thought this painting would not be an unwelcome present to the Society or an improper article to be preserved among its collection of subjects for philosophic speculation."

A little more than a century later I myself was a Simeon De Witt and was instrumental in discovering another eminent portrait painter, Howard Russell Butler, who was with me in Oregon for the total eclipse of June 8, 1918. Again the artist and the astronomer engaged in team-work, as had been done for the eclipse of 1806, Mr. Butler in the ten days before the eclipse doing the difficult task of attempting by his skill as an artist to bring out the fire and the glory immediately surrounding the moon in the inner corona, the astronomer doing the easy task of criticizing as the result of experience gained at three previous total eclipses. Most of you have seen Mr. Butler's famous paintings of the eclipses of 1918, 1923 and 1925. For the first and third of these eclipses he was with me. He was near me at the 1923 eclipse but "unusually unusual" clouds in "beautiful sunny California" prevented him from being with me. The originals of the paintings form a triptych at the American Museum of Natural History, and copies by Mr. Butler are in the Franklin Museum in this city and in the building of the National Academy of Sciences in Washington.

Andrew Ellicott (1754–1820) has been to me a most interesting character. He was of Dutch and English Quaker stock and belonged to the family that founded Ellicott City, Maryland. He was a mathematician and a surveyor and therefore an astronomer. In 1784, he was appointed member for Virginia for the group of surveyors that continued the Mason and Dixon line southward. In 1791, he began the survey, occupying two years, that gave the boundaries of the ten-mile square tract ceded by Maryland and Virginia as the site of the present District of Columbia. In 1811, Georgia invited him to run the line between that state and North Carolina. When the job was finished, Georgia refused to pay him more than his expenses for the simple reason that his line ran 18 miles farther south than Georgia's hopes. He appears to have had many stormy times in his career but he found a haven of rest in 1813 at West Point where he was professor of mathematics.

I have quoted from one of Ellicott's letters. Another was written from Lancaster, to Thomas Jefferson, president of the United States and of the American Philosophical Society, under date of December 29, 1801. Part of the letter reads: "I enclose a few astronomical observations. . . . Being now the only native of the United States left, which time has not spunged (sic) away, and who has cultivated practical astronomy for the purpose of rendering it useful to commerce, to the division of territories, and the determination of the relative positions of the different parts of our own country, I feel a desire to keep the subject alive, till succeeded by some American, whose fortune may put it in his power to be more useful, by allowing him to devote his whole time to the improvement of so important a branch of science." The observations referred to are chiefly lunar distances to check up on the lunar theory and to determine the longitude. From these and other letters, I have come to the conclusion that he had a fairly high opinion of his own importance and that he was at times a bit difficult to get along with, perhaps a bit cantankerous. It has been said of him that "He had a happier hand with the theodolite than he had with the pen."

In spite of some shortcomings Ellicott was a most careful observer. At the eclipse of 1806 he determined the difference in longitude between Lancaster and Greenwich. The value obtained exceeded that derived earlier by 17 seconds of arc. He gave the correct explanation for this difference, namely, "imperfections in the lunar tables, which appear to give the moon's longitude at the time of the eclipse at least 1′ too much. The error in latitude at the same time is almost

insensible." Also he correctly pointed out that by noting the northern and southern limits of the moon's shadow path on the earth, "observations were made for the express purpose of correcting the lunar tables." The most perfect results following this plan were at the total eclipse of January 24, 1925, when the southern edge of the moon's shadow was accurately determined at Riverside Drive in New York City. At the top of the apartment house at 96th Street and south of the street the eclipse was not total, but north of the comparatively narrow street the corona was visible and the eclipse was total. The edge of the shadow of totality was, therefore, pinned down to within two hundred and twenty-five feet on the west edge of Manhattan Island.

I have already told you that it was the Transit of Venus that put new life and new fame into the American Philosophical Society. It was the same Transit of Venus that put new money into the coffers of the Society, for fame without money is sadly handicapped. Let me read to you a portion of the minutes of the meeting on February 19, 1773, where an address was formulated:

To the Honorable the Representatives of the Freemen of Pennsylvania in General Assembly met;

GENTLEMEN, As we must always think it our Duty most gratefully to acknowledge the early Countenance and liberal Assistance, which, as a Society, we have received from former Assemblies of this Province; nothing could induce us to make further Applications for public Aid but the Necessity of our Case & a firm persuasion that the present Members of your honorable House still view our Institution in the same friendly Light as your Predecessors have done. We would not therefore repeat on this Occasion, what we have formerly taken the Liberty to represent to them, concerning the Use of *public Societies* for promoting useful Discoveries, domestic Improvements & the Reputation of those Countries where they have been established: We rather hope that we may be allowed now, as an Instance of this, to mention our own Society, which has already promoted several useful Undertakings, & by some of its Productions, especially those relating to the Transit of Venus, has extended the literary Fame of our Country to distant parts of Europe.

Distant Ages will certainly do honor to the Assembly of *Pennsylvania* for having encouraged the Observations of the Transit in this Part of the World, which in other parts (tho' indeed under many Disadvantages, & therefore with less Success) were carried on chiefly a' the Expence of crowned Heads. As these Observations could never have been undertaken by our Society, in so many places, without such public assistance, we hope the House will give us leave to observe that the erecting our different Observatories, fitting up Instruments, engraving various plates & publishing the different *Transit-Papers*, cost us (exclusive of the other part of our publication) near *Four Hundred* pounds, & have involved us in a Debt which we are wholly unable to discharge.

We therefore pray that your honorable House, etc., etc.

Who could resist such an appeal? Evidently the House of Representatives could not, for we read in the minutes of the meeting held three weeks later the following: "The House of Assembly had been pleased to pay such Regard to the petition of this Society as to order the sum of Two Hundred and Fifty Pounds to be paid by the Provincial Treasurer for further carrying on our Designs." This money was in addition to the £100 for the erection of the Observatory on State House Square.

Going back once more to the early minutes, we find the following, noted for the meeting of 1769, September 15. "Dr. Williamson delivered to the Society a paper in which he endeavored to explain the theory of the motion of the Comets, the probability of their being inhabited, etc." This paper was referred to the Committee on Astronomy with instructions that they "publish it if they think proper without reporting." The publication appeared in *Transactions* (o.s.), **1**, appendix, 27–36, 1771.

My audience today will smile at the apparently absurd idea that comets might ever be thought to be habitable. But let us stop a minute and look back. In 1715, the great Halley[4] thought that the appearances on the eastern and western edges of the sun at a total eclipse might reasonably be expected to be different, for the reason that "the eastern limb of the moon had been exposed to the sun's rays for a fortnight, and as a consequence it would be natural to expect that the *heated lunar atmosphere* might exert some absorbing effect on the solar rays while on the contrary the western edge of the moon being in darkness and cold for two weeks could exhibit no such absorbing action." Of course we now know that the moon has no atmosphere. A half-century or more later, one of the greatest men in observational astronomy of all time thought that the dark spots on the sun might be openings into the cooler portions of the sun that even might be habitable. To Herschel, astronomy owes the great reflecting telescopes, to him we owe the discovery of the planet Uranus,

[4] *Philos. Trans. Roy. Soc.*, **29**: 248 (1715).

and to him and to his son Sir John we owe tremendous advances, particularly in stellar astronomy. Herschel's telescopes (judged by modern standards) were very crude affairs. It was not until 1817 that the art of glass-making had advanced enough to permit Fraunhofer to construct the "great Dorpat refractor" as it was called, with the then unprecedented aperture of 9½ inches.

This review of astronomy in the early years of the Society would not be complete unless I called your attention to the enormous difference in an observatory then and now. At the present time an observatory may mean a 200-inch telescope and six millions of dollars. In the eighteenth century an observatory meant a structure costing a few hundred dollars, with a telescope small and crude, according to modern standards, the telescope usually fixed in the meridian, chiefly to determine time and to assist in surveys of the virgin country. Most of these, like that on the State House Square, had a transitory existence. The first observatory belonging to a college was not erected near Philadelphia or New York or anywhere in the North, but south of the Mason and Dixon line. I have told you of the all-round ability of John Ewing, provost of the University of Pennsylvania. To illustrate the fact that Virginia never takes second place to any state in the Union (I should add that I am not a Virginian by birth) I would like to tell you a little about a contemporary of Jefferson, who with Jefferson was elected a member of the American Philosophical Society on January 27, 1780: James Madison (1749–1812). At the age of 28 he became the president of the College of William and Mary, the second oldest college in the United States, and later he was the first bishop of the Protestant Episcopal Church in Virginia. In the year 1780, in November, he sent to Rittenhouse observations of the eclipse of Jupiter's satellites and the value of the longitude from Paris derived therefrom. In Vol. 3 of the *Transactions*, page 150, reference is made to the "observatory" and to observations made of a lunar eclipse on November 2 and of a Transit of Mercury on November 5, 1789.

The second observatory in this country was likewise not in the North but in the South, at the University of North Carolina. Joseph Caldwell was born in 1773 and graduated from Princeton when only 18 years old. At the age of 23 he went to Chapel Hill as professor of mathematics and at the age of 31 he became president of the university. In the year 1824 the trustees of the university had enough confidence in their president to give him $6600 and send him to Europe to buy books and laboratory apparatus; the money spent was about equally divided between books and instruments. The equipment was chiefly astronomical and consisted in a meridian transit telescope, an altitude and azimuth telescope, a refracting telescope, a good clock with a mercurial pendulum, a sextant, a Hadley's quadrant and a portable reflecting circle. An observatory was built in 1830 and 1831, largely at President Caldwell's own expense. Its cost is given as $430.29½. Unfortunately, the construction was not very solid, and the roof leaked. After President Caldwell's death, the building deteriorated and it burned in 1838. The instruments, however, were saved. My friend, Dr. MacNider (of the University of North Carolina), writes me, "I wish so much you could see these beautiful old instruments. The brass is one of the softest, loveliest things in the form of a metal that I have ever looked at." Some of Sherman's soldiers on their march from the sea passed through Chapel Hill. They too admired the lovely finish of a telescope found on a dusty shelf in one of the laboratories. In picking up the telescope tube to examine it more closely, they found something loose inside, which turned out to be two gold watches placed by professors for safe keeping in the securest place imaginable. Needless to say, the watches were immediately appropriated. When the soldiers had left the laboratory, the professors returned to wind up their watches, only to find no trace of them. The loss was at once reported to the commanding officer. The story does not end there, and furnishes another triumph for the course of true love. It is said that the commandant was in love with the daughter of the president. The gold watches were promptly recovered and were returned to their rightful owners.

In the North, Yale University in 1830 secured a 5-inch Dollond refractor of 10-foot focus. There was no observatory. The telescope was pulled around in the Athenaeum Tower and pointed through one of the windows. Unfortunately, the windows were low and no object more than 30 degrees above the horizon could be observed. Similarly, Harvard College[5] possessed astronomical instruments from an early

[5] W. I. Milham, "Early American Observatories," *Popular Astronomy*, **45**: 523 (1937).

date but no astronomical observatory. Harvard College Observatory was established in October, 1839.

As might have been expected, astronomy, the so-called queen of the sciences, played a most important part in the early years of the American Philosophical Society for Promoting of Useful Knowledge. And this was so, largely because astronomy more than any other science of 150 to 200 years ago was useful in the promotion of useful knowledge. Today when some of us can get along very well with a very inexpensive watch, we perhaps forget that before the days of the electric telegraph the only manner of ascertaining exact time was to make astronomical observations. Although exact time and topographic surveys were necessary as useful knowledge, the only astronomers whose names have lived throughout the two centuries are those who pursued knowledge for the useful purpose of gaining information about matters with no immediate practical application to business or to mundane affairs.

The history of astronomy of our Society, revealed through manuscripts in our valuable library in the Drexel Building across the street, includes a few outstanding names of men who gained valuable knowledge regarding the distance to the sun or who progressed a step further toward solving Nature's secrets, through observations of eclipses of the sun or moon or of Jupiter's satellites.

And now in closing I wish to state that I am not vain enough to believe that you have found my review of astronomy "both entertaining and instructive," to quote the words of David Rittenhouse, even though I did find my brief excursion into the early history most entertaining and instructive to myself.

SOME EARLY AMERICAN PHYSICISTS

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(*Read February 13, 1942, in Symposium on the Early History of Science and Learning in America*)

In my college days at the University of Pennsylvania, when as an undergraduate I attended the chapel exercises each morning (for it was then required), I would at times let my glances stray over the brightly colored windows which had been erected in memory of distinguished trustees, professors and patrons of the institution. There in the most commanding position was the great window dedicated to the founder of the University, bearing as its motto the well known epigram of Turgot, *eripuit caelo fulmen sceptrumque tyrannis.* Flanking it on one side was the memorial to one of its earliest professors, the noted astronomer David Rittenhouse, recalling his multiform activities by effigies of a telescope, a coinage press, and the celebrated orrery. On the other side was the window commemorating Alexander Dallas Bache, with symbols of his geodesic and magnetic discoveries. Each of these in his turn was President of the American Philosophical Society, and each contributed notably to "the Early History of Science and Learning in America."[1]

But one of the three, Alexander Dallas Bache, was honored only a twelvemonth ago by this Society with a sympathetic study of his life and activities in the many organizations to which he contributed so much.[2] Moreover, Bache's scientific work lies beyond the period considered in this review.

Turning now to the second of the triumvirate, David Rittenhouse, it must be admitted that he is noted chiefly as an astronomer, and therefore, it might be thought, should not be included here. His astronomical achievements, so intimately related to the history of this Society, have been ably discussed by another speaker. But in the early days of American science the realm was not yet divided into those water-tight compartments which only recently are once more beginning to overflow into one another, to the mutual benefit of all.

And Rittenhouse was not only an astronomer; he was also a mechanical genius and a physicist.[3] A large proportion of his published papers treated of physical subjects. We find him devising a metallic thermometer, and somewhat later a hygrometer. His skill as a surveyor was shown in many important projects carried out with instruments which he himself designed and fabricated. The accuracy of his clocks, for which he is famous, was due not only to his practical skill but also to a thorough acquaintance with the physical theory involved. Two of his last papers bear upon the improvement of timepieces, and he is said to have written much more on this subject, which unfortunately has not survived. Quoting his words,[4] "Pendulum clocks . . . have been made to measure time with astonishing accuracy; and, if there are still some causes of inequality in their motions, the united efforts of mechanism, philosophy and mathematics, will probably, in time, remove them."

Throughout his life Rittenhouse was intensely interested in optics. When only twenty-four, during the French and Indian War, he wrote to his brother-in-law,[5] "I have not health for a soldier, and as I have no expectation of serving my country in that way, I am spending my time in the old trifling manner, and am so taken with optics, that I do not know whether, if the enemy should invade this part of the country, as Archimedes was slain while making geometrical figures on the sand, so I should die making a telescope." He always maintained his interest in the subject. Many years afterward he was eagerly planning a course of lectures on optics, but for some reason they were not delivered.

His most notable optical achievement, read before this Society in 1786,[6] was an account of a dif-

[1] Now one seeks in vain for these memorials. The hall that was once a chapel is so no longer, but is transformed into a laboratory of geology, and the decorative windows are replaced by clear glass and, let us hope, stored away for a future resurrection.

[2] *Proc. Amer. Philos. Soc.*, **84**, 125 (1941).

[3] For a fuller discussion of this phase of Rittenhouse's career see an article by Thomas D. Cope, *Jour. Frank. Inst.*, **215**, 287 (1933).

[4] *Trans. Amer. Philos. Soc.* (o.s.), **4**, 26 (1799).

[5] Barton, *Memoirs of David Rittenhouse*, 105n. (1813).

[6] *Trans. Amer. Philos. Soc.* (o.s.), **2**, 202 (1786).

fraction grating. In the construction of this he used the same method that was employed a third of a century later by Fraunhofer, who has usually received the credit of being the first to make this device.[7] Rittenhouse correctly ascribed the observed phenomena to the inflexion (or as we now say, diffraction) of light, and noted their agreement with some analogous observations of Newton; but as the experiment antedated by fifteen years the rebirth of the wave theory, he could give no adequate interpretation. He closes his paper with the remark: "By pursuing these experiments it is probable that new and interesting discoveries may be made, respecting the properties of this wonderful substance, light. . . . But want of leisure obliges me to quit the subject for the present." It is to be regretted that he was prevented by his many duties and feeble health from continuing a line of research that might have anticipated some of the labors and the glories of Young and Fresnel.

Rittenhouse does not seem to have devoted much attention (publicly at least) to the new science of electricity, though he twice discussed before the Society cases of the effect of lightning on houses in the vicinity. In the allied field of magnetism we find an instance of his clear-sighted visualization of a physical problem in a paper read in 1781. In this he proposed what is now known as the molecular theory of magnetism long before the German physicist Wilhelm Weber—to whom the credit is usually given—was born.

Can any statement of the theory be clearer than his own?[8]

> I suppose then, that magnetical particles of matter are a necessary constituent part of that metal which we call iron . . . These magnetical particles I suppose have each a north and a south pole, and that they retain their polarity, however the metal may be fused or otherwise wrought. In a piece of iron which shews no signs of magnetism these magnetical particles lie irregularly, with their poles pointing in all possible directions, they therefore mutually destroy each others' effects. By giving magnetism to a piece of iron we do nothing more than arrange these particles, and when this is done it depends on the temper and situation of the iron whether that arrangement shall continue, that is, whether the piece of metal shall remain for a long time magnetical or not.

The most ingenious achievement of Rittenhouse was his famous orrery or mechanical model of the solar system. While the purpose of this instrument was the elucidation and prediction of astronomical phenomena, its construction was a triumph of mechanical genius. It was not simply an elaborate toy. As was said by its designer,[9] "I would have my Orrery really useful, by making it capable of informing us, truly, of the astronomical phænomena for any particular point of time; which, I do not find that any Orrery yet made, can do." And the Rittenhouse orrery was designed to accomplish this over a range of five thousand years, in the past or in the future.

The history of the orrery is not without interest. Rittenhouse may have originally hoped to dispose of it to the Colonial Legislature for the College. But it seems that Dr. Witherspoon of the College of New Jersey at Princeton stole a march on the sister institution and bargained for it before its completion, much to the mortification of Provost William Smith, who in a letter[10] "regretted that he [Rittenhouse] should think so little of his *noble* invention, as to consent to let it go to a village." However, Rittenhouse agreed to construct a second orrery for the other College, and this was publicly exhibited in the winter of 1770–71. The Pennsylvania Legislature in admiration rewarded the inventor, and at once ordered a third, of double the dimensions—perhaps to checkmate the officious neighbor. This scheme was never carried out, doubtless because of the growing intensity of the political struggle with the mother country. Of the two actually constructed, the first, perhaps as an act of poetic justice, has completely disappeared. Its disintegration began at the time of the occupation of Princeton during the Revolution by the opposing forces, when the soldiers appropriated the wheels as interesting souvenirs.[11] The other survived the chances of war, and still remains in the possession of the University of Pennsylvania.[12]

One more abortive incident may be narrated as an epilogue, since a share in it is borne by this Society. It is recorded in its minutes[13] that at a meeting held January 13, 1783, twelve years after the completion of the orreries, "Jefferson moved (Reed seconded) that Rittenhouse should make

[7] The priority of Rittenhouse in the construction of a grating was brought to light by Maurice J. Babb in 1932. See article by Thomas D. Cope in *Jour. Frank. Inst.*, **214**, 99 (1932).

[8] *Trans. Amer. Philos. Soc.* (o.s.), **2**, 178 (1786).

[9] Barton, *op. cit.*, 194.

[10] *Ibid.*, 216n.

[11] It seems that the chief culprits were the American militia, according to a letter of President S. S. Smith, quoted by Barton, 342n.

[12] At the time of the Rittenhouse Bicentenary celebration in 1932 it was renovated and for some years placed on exhibition at the Franklin Institute in this city.

[13] *Proc. Amer. Philos. Soc.*, **22**, 116, 118 (1885).

an Orrery to be presented to his Most Christian Majesty. Com. of three to carry this into effect." On March 6 is the laconic entry, "Rittenhouse will make an Orrery." And finally on September 26 is the minute, "Letter from the French Minister to Rev^{d} D^{r} White that the King would accept the Orrery, and 'by his Royal Patronage excite an Emulation between the Literary Societies of France and the United States.'—Ordered that D^{r} White . . . acknowledge . . . 'the high sense the Society entertain of the Honor done them by his Majesty,' &c." Nothing further appears and nothing seems to have been done. King Louis soon had too many troubles of his own to worry about orreries.

Rittenhouse was looked upon by his fellows as, after Franklin, the foremost scientist of the country. His abilities were highly esteemed abroad, though, as has been seen, the recognition was limited to his astronomical work. On the death of Franklin he was at once elected to the presidency of this Society, and when six years later he passed away at the age of sixty-four, his successor, Thomas Jefferson, in accepting the same office summed up his character in the words:[14] "Genius, Science, modesty, purity of morals, simplicity of manners, marked him one of Nature's best samples of the Perfection she can cover under the human form. Surely, no Society, till ours, within the same compass of time, ever had to deplore the loss of two such members as Franklin and Rittenhouse."

And now, with reverence, and with hesitation withal, I approach the shrine of the founder of this Society.—In the century and a half that has passed since the death of Franklin so much has been written and spoken concerning this great philosopher that one despairs of discovering or saying anything new. Beginning with the eulogium pronounced by Provost Smith in 1791, this Society honored him with a commemorative meeting on the centenary of his death and again on the bicentenary of his birth. On each of these occasions there was presented a paper devoted to his scientific work. And only two years ago we were invited to the Franklin Institute to "Meet Dr. Franklin" in a series of brilliant addresses which subjected to critical analysis every phase of his career.

And yet, upon this occasion and in these halls, it would be almost a sacrilege to fail to treat of the work of our first great scientist, who established abroad the scientific reputation of America, and of Philadelphia in particular, and who laid so firmly the foundations of electrical science that two centuries later these foundations support without crumbling the elaborate superstructure of modern theory. To evaluate his true position in the history of science, can we do better than follow the judgment of our Dean of physical science, Robert A. Millikan? From the time of the birth of Copernicus to the close of the eighteenth century, Millikan[15] lists only six scientists who may be called—in the apt phrase of Van Doren—not merely great men but *great* great men: Copernicus, Galileo, Newton, Huygens, Franklin and Laplace.

It is sometimes held that the scientific interests of Franklin were subordinate to his other activities and that he turned to science merely as a relaxation from more important pursuits. Quite the contrary is the case. Of course he derived great entertainment from his researches. What scientist does not? Shall we not rather classify him as a scientist who dabbled in politics? That his "dabbling" resulted in immeasurable benefits to his country as symbolized by his signatures to the Declaration, the Peace Treaty and the Constitution, does not obscure his lifelong interest in scientific work. When only twenty his journal narrating his voyage home from London is replete with acute observations ranging from the methods of cultivating oysters to eclipses of the sun and moon. And in the year before his death, in the midst of almost constant pain, we find him experimenting upon his range of hearing under varying conditions. The experiment itself was perhaps trivial, but Franklin's sense of scientific accuracy in observation is shown in the care with which the data were recorded.

And his interest in science was universal. It may be sufficient to point out that in his writings may be found discussions of practically all the sixteen topics included in the present symposium. Even in the enumeration of his physical studies we see how widespread was his interest. He studied the cold caused by evaporation, and the condensation of dew. He noted the difference in conductivity of various materials, and their different powers of absorbing solar heat, recommending light colored clothes for summer wear. He studied the effect of oil in smoothing the surface of water, carrying about with him for this purpose in his daily walks some oil in a hollow of his walking-stick. He devised an efficient fire-

[14] *Ibid.*, 251.

[15] *Jour. Frank. Inst.*, **232**, 407 (1941).

place which ever since has been known as the Franklin stove. He witnessed with great interest the first balloon ascensions in Paris, and sent a description of them to this Society; and when a little later a balloon crossed the English Channel, Franklin received the first airmail letter from London—a missive which philatelists would doubtless prize above the rarest stamp in their collections. He invented or improved the glass harmonica, which became popular in Europe for many years under the name of the "musical glasses." He was the first scientist to study the characteristics of the Gulf Stream and to map its course. He invented for his own use bifocal glasses, and found, he says, "I understand French better by the help of my spectacles." After his final return to America he presented before this Society papers (which he had written during the voyage) on the causes and cure of smoky chimneys and on the propulsion of boats by machinery. He attended and presided over meetings of the Society until six months before his death.

But of course it is upon his work in electricity that the fame of Franklin as a scientist chiefly rests. This has been so ably treated by Dr. Millikan in a recent article[16] that it would be superfluous to attempt an analysis here. Suffice it to say that in the little collection of letters (less than a hundred pages) published in London in 1751—a copy of which is literally worth its weight in gold—we find, established by ingenious and searching experiments, the fundamental theory which brought order to the confused mass of electrical phenomena. As Van Doren happily expresses it: "He found electricity a curiosity and left it a science."[17]

In carrying on his electrical experiments, Franklin did not work alone. As he tells the story in his introductory letter reporting them,[18] "Your kind present of an electric tube, with directions for using it, has put several of us (i.e. of the *Library-Company* . . .) on making electrical experiments, in which we have observed some particular phænomena that we look upon to be new." And in a letter written shortly afterward to Thomas Penn, thanking him for his gift of an electrical machine, are the words,[19] ". . . some of our Members who employ themselves in such Researches may, by it's Assistance, shew that this fresh Mark of your Favour has been judiciously, as well as generously bestowed. . . ."

Among the many who shared with Franklin himself the pleasure of witnessing his experiments—he tells how his "house was continually full . . . with people who came to see these new wonders"—there were a few who took a more serious part in the studies. Franklin was doubtless the leader, but others contributed valuable suggestions, which were given due credit. Thus perhaps was formed the first Physics Colloquium in America.

Of the members of this "Colloquium," the names of only three have come down to us. Two of these, Thomas Hopkinson and Philip Syng, made minor though important contributions to the investigations, but there seems to be no record of their scientific work except the footnotes in Franklin's book. Hopkinson was President of the American Philosophical Society when it was organized in 1743, but died before the famous kite was flown. Syng had been a member of the Junto and was active in many civic institutions. For a time he was treasurer of this Society. He records himself a silversmith, and made the inkstand which was used at the signing of the Declaration of Independence.

The third of the trio deserves much more attention. Franklin introduces him to us in these words:[20] "Among these [co-workers] the principal was Mr. Kinnersley, an ingenious neighbor, who, being out of business, I encouraged him to undertake showing the experiments for money, and drew up for him two lectures, in which the experiments were rang'd in such order, and accompanied with such explanations in such method, as that the foregoing should assist in comprehending the following." Let us inquire as to the personality of this "ingenious neighbor."

Ebenezer Kinnersley, the son of a Baptist minister, was brought to America in childhood and spent his youth and early manhood in Lower Dublin Township, near Philadelphia. In his twenty-eighth year he married and brought his young wife to the city, where he probably earned his living by teaching school while studying for the ministry. Though ordained in the Baptist Church he did not obtain a charge, and about this time made the acquaintance of Franklin. They became close friends, and in 1753 Kinnersley was

[16] *Loc. cit.*

[17] A new edition of Franklin's book, *Experiments and Observations on Electricity*, with a valuable critical and historical introduction by I. Bernard Cohen, has recently appeared (Harvard Univ. Press, 1941).

[18] *Experiments and Observations*, fifth ed., (1774).

[19] *Minutes*, Library Company of Philadelphia, Sept. 14, 1747.

[20] *Writings*, 1, 417.

successfully recommended by Franklin to the post of English Master of the new "Academy and Charitable School" which had been opened a short time before. Two years later the institution was raised to the rank of a college, and Kinnersley became "Professor of the English Tongue and of oratory."

The appearance of a scientist as Professor of English deserves comment. Franklin was probably anxious to have some one on the faculty in sympathy with his views on the importance of instruction in English, so as to counteract overemphasis on the classics. Science was not taught at the Academy before the arrival of the new Provost, Dr. William Smith, and there was no professorship of Natural Philosophy formally established until nine years later. The vacancy in the English School was for Franklin an opportunity not to be missed. Moreover it is likely that Kinnersley's lectures had already earned him a favorable reputation as a public speaker.

It is interesting to note the duties assumed by Kinnersley in his mastership. We read in the Constitution of the Academy[21] that "The *English Master* shall be obliged, without the Assistance of any Tutor, to teach Forty Scholars the *English Tongue* grammatically and as a Language; and at the Same Time, according to the best of his Capacity, to instruct them in *History, Geography, Chronology, Logick* and *Oratory*." The chair seems to have been somewhat of a settee. Further, when the College opened its dormitory building, Kinnersley (described in the prospectus as "a Gentleman of regular and exemplary Life") was appointed Steward with additional duties, some of which we learn from a minute of the Trustees which states:[22] ". . . that the care of the Boys with respect to their Linnen, Combing their Heads and other matters, in which the younger part of them could not be trusted to themselves, had always been considered as part of the Steward's Duty. . . ." Such was the life of our English Professor and scientist.

Of Kinnersley's success in his English chair we have no record. He seems to have made a favorable impression on his students, one of whom recalls him[23] as "a large, venerable looking man of no great general erudition though a considerable proficient in electricity," and another speaks of him[24] as "the truly respectable Ebenezer Kinnersley, [who] exercised great care over his pupils." If this is damning with faint praise, it is at least an improvement over the reputation of his predecessor, David James Dove, who was remembered by a former pupil[25] as "a sarcastical and ill-tempered doggerelizer, and was called *Dove* ironically—for his temper was that of a hawk, and his pen was the beak of a falcon pouncing on innocent prey."

However, the reputation of Kinnersley was gained not from his English chair but from his scientific labors. A worker with Franklin from the first, he also carried out many independent investigations, the results of which he duly communicated to his friend, and so aided greatly in the advance of the science. He seems to have continued his experiments until failing health obliged him to resign his professorship and retire from active life in 1773. He died five years later.

In addition to his researches he made, as we are told by Franklin, considerable reputation as a lecturer. His lectures were illustrated by many ingenious and spectacular experiments. They were delivered throughout the northern Colonies, and many times in Philadelphia. He even gave a course during one of his health-seeking trips to the West Indies. For these and for his researches he gathered what was probably the finest collection of electrical apparatus at that time in America, which, after his retirement, was acquired by the College.

Kinnersley seems to have been of a retiring disposition and to have left few records of his activities. He published little himself, and the value of his achievements was made known to the world mainly by Franklin's testimony and by the letters exchanged between them and published in Franklin's book. A device showing the heating effect of the electric spark, described in one of the letters, is still known as Kinnersley's Thermometer. A study on the behavior of the electric eel, which was authorized by this Society in 1773, was not published until long after his death.[26] Nevertheless, the value of his investigations was recognized by scientists here and abroad. Priestley in his *History* frequently reports Kinnersley's work with appreciation and sums it up with the esti-

[21] Montgomery, *History of the Univ. of Penna., 1749–1770*, 48.

[22] *Ibid.*, 448.

[23] Graydon, *Memoirs*, 16 (1811).

[24] Frank, *Memoirs of Lindley Murray*, 8 (1827).

[25] Watson, *Annals of Philadelphia*, 1, 561 (1877 ed.).

[26] *Phila. Med. and Phys. Jour.*, 1, Pt. 2, 96, 159 (1805). The article was reported by Rittenhouse, but Kinnersley clearly was the leader.

mate, "If Kinnersley continues his electrical inquiries his name, after that of his friend, will be second to few in the history of science."[27]

Among the group who gathered with interest around Franklin's electric tube we look in vain for one whose presence might surely have been expected. One of Franklin's earliest Philadelphia acquaintances, who lived under the same roof with him for two years, an original member of the Junto, of the Library Company and of the American Philosophical Society, known as an excellent mathematician and inventor, Thomas Godfrey would have been well worthy of such a company. Nor is he unworthy of a place in this survey.

Godfrey, two years Franklin's senior, had learned the trade of a glazier, and his work was seen (or shall we rather say, looked through) in the windows of the State House and many of the colonial mansions surrounding the city. He is pictured thus by his patron and admirer, James Logan:[28] "A young man born in this country . . . by trade a glasier [*sic*], who had no other education, than to learn to read and write, with a little common arithmetick, having in his apprenticeship, with a very poor man of that trade, accidentally met with a mathematical book, took such a fancy to the study, that by the natural strength of his genius, without any instructor, he soon made himself master of that, and of every other of the kind, he could borrow or procure in *English:* and finding, there was more to be had in *Latin* books, under all imaginable discouragements, applied himself, to the study of that language, till he could pretty well understand an author on these subjects, after which, the first time I ever saw or heard of him, to my knowledge, he came to borrow sir *Isaac Newton's* principia of me. Inquiring of him hereupon who he was, I was indeed astonished at his request, but after a little discourse, he soon became welcome to that or any other book I had."

Franklin's description is somewhat less laudatory:[29] "A self-taught mathematician, great in his way, and afterward inventor of what is now called Hadley's Quadrant. But he knew little out of his way, and was not a pleasing companion; as, like most great mathematicians I have met with, he expected universal precision in everything said, or was for ever denying or distinguishing upon trifles, to the disturbance of all conversation."

In spite of this unfavorable opinion, Franklin tells us that when a young man he boarded with Godfrey who, he says,[30] "lived in part of my house with his wife and children, and had one side of the shop for his glazier's business, though he worked little, being always absorbed in his mathematics." The joint tenancy lasted two years until Franklin set up housekeeping on his own. Their association, however, still continued; Godfrey published, and Franklin printed, three almanacs for the years 1730, 1731 and 1732. These were the immediate precursors of the famous "Poor Richard" series which began the next year. When the Library Company was founded Godfrey became one of its first Directors. And later, as was said, he was one of the nine original members of the American Philosophical Society.

His trade as a glazier may not have been very lucrative—the Academy with its incipient "rowbottoms" and breaking of windows was still in the future—or perhaps he sought more intellectual pursuits. As noted above, he published almanacs[31]—always a profitable venture—and for a time was a schoolmaster. As a surveyor he took part in one of the first surveys of Delaware Bay. He died in 1749 when only forty-five years old.

Shall we then attribute Godfrey's absence from Franklin's coterie to his contentious disposition which made him undesirable as a participant? Or shall we more charitably assume that his early death, so soon after the time when the experiments were begun, suggests a poor state of health which might have prevented his attendance? It is now impossible to say.

To students of literature Godfrey is known as the father of Thomas Godfrey, Jr., who was the author of the first American tragedy, *The Prince of Parthia,* first performed in Philadelphia in 1767. To the scientific world his fame rests on the invention of the Mariner's Quadrant. For though he is always spoken of as a mathematician, little record of his work in that field has survived.

The invention of the Quadrant, as narrated by Watson, Philadelphia's noted annalist, following a tradition in the Logan family, was made as fol-

[27] *History of Electricity,* 4th ed., 179 (1775).—In 1873 a memorial window dedicated to Kinnersley was placed in the new building of the University of Pennsylvania, but this, together with the other commemorative windows, has since been removed.

[28] Bradford's *Amer. Mag.,* 1, 475 (1758).

[29] *Writings,* 1, 299.

[30] *Ibid.,* 1, 309.

[31] These were continued for some years from the press of Bradford, the last being for the year 1737.

lows:[32] "While engaged at his business on the premises at Stenton—J. Logan's place—accidentally observing a piece of fallen glass, an idea presented [itself] to his reflecting mind, which caused him to quit his scaffold and to go into Mr. Logan's library, where he took down a volume of Newton. Mr. Logan entering at this time and seeing the book in his hand, inquired into the motive of his search, when he was exceedingly pleased with Godfrey's ingenuity, and from that time became his zealous friend." The story, though often quoted, bears obvious marks of improbability. A more plausible and not less picturesque tale, of at least equal authority, is told as follows:[33] "Godfrey . . . was one day engaged in replacing a pane of glass in the window of a house on the North side of Mulberry street. A girl who had filled a pail with water, at a pump that stood opposite, placed it on the sidewalk, and Godfrey on turning towards it, saw the sun which had been first reflected from the window on which he had been at work, into the bucket of water, and a second time from the surface of the water to his eye. His philosophic mind seized at once upon the observation, and the principle was thus applied to the construction of an instrument with which he could draw the sun down to the horizon by means of a contrivance incomparably superior to any that had ever been used for the purpose of ascertaining angular measurements." Whatever the details, it is certain that Godfrey as early as 1730—in his twenty-sixth year—had brought his instrument into a practical form. A description of the invention was sent to England by James Logan in 1732,[28] but unfortunately before it reached the Royal Society an account of the invention of the same device by John Hadley had been presented to that body. The instrument therefore is usually called Hadley's Quadrant. There was a considerable controversy as to the relative claims of the two inventors. The story goes that, to compensate for its previous recognition of Hadley, the Royal Society (or perhaps some of its members) acknowledged Godfrey's independent discovery by sending him a gift of household furniture of the value of two hundred pounds.[34] The extraordinary form of this reward may well cause wonder. It is explained in the recorded gossip as due to his being "apt to indulge in intemperate drinking." If this interpretation is valid, it throws a flash of light, though a lurid one, upon the life of this strange genius.

This review has sought to sketch briefly the lives and work of some of the men who were the first to employ themselves in the pursuit of the physical sciences in America. To preserve the unities of place and time it has been limited to the city of Philadelphia and to the span of years compassed by Franklin's residence here. The justification of this restriction may be found in the fact that Philadelphia was in those days recognized to be the center of the intellectual life of the Colonies. How far the formation of this Society was the cause, and how far the effect of that preeminence, we need not inquire. It is sufficient to remark that almost all the scientific men of the period were either active or corresponding members of the Society.

The work of some of the representative physicists who succeeded these pioneers—of Robert Hare, Joseph Saxton, Alexander Bache and, not the least, Joseph Henry—was ably narrated by my esteemed teacher, George F. Barker, at the one hundred and fiftieth birthday of the Society forty-nine years ago. It need only be said here in closing that in his comprehensive address, entitled *Electrical Progress since 1743*,[35] the story of American physics was brought down well past the middle of the last century, reaching the time when it is no longer appropriate to speak of the *Early* History of Science and Learning in America.

[32] *Annals of Philadelphia*, 1, 528.

[33] The story is printed in the *Western Carolinian* of Salisbury, N. C., for August 21, 1821, as copied from the Petersburg (Va.) *Intelligencer*. The writer there states that he obtained it from his grandfather, a contemporary of Godfrey. This was probably John S. Hutton, a silversmith of Philadelphia.

[34] There seems to be no record of any formal action taken by the Royal Society in recognition of Godfrey. The story of the gift of furniture is found in Miller's *Brief Retrospect of the Eighteenth Century*, 1, 480 (1803), and is there given as hearsay evidence.

[35] *Proc. Amer. Philos. Soc.*, 32, 104 (1894).

A REVIEW OF PAPERS ON METEOROLOGY AND CLIMATOLOGY PUBLISHED BY THE AMERICAN PHILOSOPHICAL SOCIETY PRIOR TO THE TWENTIETH CENTURY

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(Read by F. W. Reichelderfer, February 13, 1942, in Symposium on the Early History of Science and Learning in America)

AMONG the meteorological and climatological papers published by the American Philosophical Society before the twentieth century, some constituted definite advances in our knowledge of the weather and its effects, and it is to these papers that the present review is limited. They are relatively few, since most of the early literature on these subjects, wherever published, contributed little of permanent value, however interesting at the time. The papers here selected would not constitute a logical sequence, no matter how arranged, and therefore they are considered in their chronological order. We disappointedly look in vain for the names of Farmer Washington and Farmer Jefferson whose private weather diaries are both interesting and valuable.

The following two papers, though published after the close of the nineteenth century, are here included, since they concern work done at an earlier date by Franklin:

Abbe, Cleveland, "Benjamin Franklin as Meteorologist." *Proc. Amer. Philos. Soc.*, **45**: 117–128, 183 (1906).

Although this paper is a pleasant ramble over a large portion of Franklin's life, only those parts will here be considered that are of meteorological interest.

In his detailed diary kept during a voyage from London to Philadelphia in 1726, Franklin notes that he saw a lunar rainbow on August 30. This observation is of interest chiefly because the phenomenon mentioned, the lunar rainbow, is seldom seen except by persons in particularly favored localities, such as Honolulu and places near-by.

Reference is made also to Franklin's kite experiments by which the electrical nature of lightning was definitely established, but the experiments themselves are not discussed in detail, at any rate not nearly so fully as may be found elsewhere.

Some account is given of the weather predictions in Franklin's *Poor Richard's Almanac* and in a manner that actually is much more humorous than Abbe probably realized, since it shows a conflict, in this case justified, between "thrift" and science. This famous almanac, like all other almanacs of that day that had any appreciable sale, contained, and necessarily contained, long-range forecasts of the weather. Franklin was shrewd enough as a business man to include these forecasts in his almanac and at the same time honest enough as a scientist to make fun of them and say that he himself had no trace of confidence in them.

A brief account also is given of Franklin's correct explanation (already well known) of why the lowest temperature in winter does not occur until several weeks after the time of the shortest day; that is, because heat is slowly conducted out from the earth where it had accumulated through the summer.

A considerable account is included of Franklin's observations of a persistent dry fog over Europe in the fall of 1783, which greatly diminished the intensity of the solar radiation at the surface of the earth. Franklin speculated that this fog might have been due to the occurrence of volcanic dust in the upper air and that the decrease of intensity of the radiation received would lead to decrease of the atmospheric temperature. These observations certainly were correct as was also the idea that the temperature of the air would be decreased by every such "dry" fog whenever and wherever it might occur. His reasoning, however, as to why these low temperatures should occur at such times was faulty, but certainly as good as could possibly have been offered in his day and even later until certain then unknown facts in regard to the transmission of radiation through a dust-laden atmosphere were discovered.

Franklin also correctly infers that the air at the level of our highest clouds must be very cold but he offers no explanation as to how it ever got cold or, once cold, could remain so in the presence of sunshine. Here again we must not criticize Franklin, since this phenomenon seldom is cor-

rectly explained even now by the ablest scientists who have occasion to mention it.

Davis, William Morris, "Was Lewis Evans or Benjamin Franklin the First to Recognize that Our Northeast Storms Come from the Southwest?" *Proc. Amer. Philos. Soc.*, **45**: 129–130, 183 (1906).

In this paper Davis shows that the important discovery that in the Eastern United States general or cyclonic storms move mainly toward the northeast, although at the same time the surface winds themselves are in the opposite direction, was made by Franklin in 1743, through his studies of the progress of a lunar eclipse; this eclipse he found was visible in Boston but not in Philadelphia, owing to the occurrence there of clouds associated with a general storm that later appeared at Boston. The claim that the discovery of the eastward movement of storms was made by Evans seems, according to Davis, to rest on a notation on a map prepared by Evans, which notation, Davis shows, in all probability was added to the map by Franklin himself as editor and publisher. At any rate it subsequently was removed, and does not occur in later editions of Evans' work.

Perkins, John, "Conjectures Concerning Wind and Waterspouts, Tornadoes and Hurricanes." *Trans. Amer. Philos. Soc.* (o.s.) **2**: 335–347 (1786).

The value of this paper rests wholly in the conclusive evidence it contains to show that the water in a waterspout, above the spray roundabout it, is fresh-water rain and not water that has been drawn up into the spout from the salty ocean beneath, an erroneous idea that was very commonly believed before the date of this paper. The rest of the paper, dealing with tornadoes and hurricanes, is essentially extrapolated speculation and almost wholly erroneous.

There appears to be no biographical sketch of Perkins available in any of the places where such accounts usually occur.

Dunbar, William, "Meteorological Observation for One Year from Jan. 31, 1800, Made at The Forest Four and a Half Miles Northeast of the Mississippi." *Trans. Amer. Philos. Soc.* (o.s.) **6**: 9–24 (1809).

This rather detailed meteorological record, recommended by Thomas Jefferson for presentation to the Philosophical Society, extends from the end of January 1800 to the end of January 1801. It gives in considerable detail the strength of the wind, as light Zephyr, brisk breeze, very strong wind, tempest; it also contains an account of the barometric pressure day by day, the temperature, state of weather, and precipitation at Dunbar's plantation, "The Forest," 9 miles south of Natchez and 4 miles east of the Mississippi. The interest in this report is owing primarily to the early date of the observations, and the value of the observations is due largely to the evidence they give as to whether or not appreciable climatic changes have occurred in that region since they were taken. These were the earliest meteorological records in the Mississippi Valley.

Mr. Dunbar was of a titled Scottish family, educated in Scotland and England; came to this country as a young man; amassed, lost and again amassed a considerable fortune, especially around Natchez, Mississippi. He was a man of very wide scientific interest, a member of the American Philosophical Society on the nomination of Thomas Jefferson.

Collin, Nicholas, "Observations Made at An Early Period on the Climate of the Country About the River Delaware, Collected from the Records of the Swedish Colony." *Trans. Amer. Philos. Soc.* **1**: 340–352 (1818).

This article contains no original observations by the author, but is an excellent summary of the meteorological and climatological records made at the Swedish Colony near the present site of Wilmington, Del. These were the first records of meteorological phenomena ever made at any place in the Americas. This paper also gives many interesting accounts of this colony and the region roundabout.

Mr. Collin was a French theologian, born in the early part of the eighteenth century. He is said to have written some religious articles, but nothing of a scientific nature except the translation here reviewed.

Bache, Alexander Dallas, "Meteorological Observations During the Severe Gust of April 2, 1841." (Verbal comm.) *Proc. Amer. Philos. Soc.* **2**: 18, 56–57 (1841).

This is a detailed and most excellent account of the sequence of weather phenomena that accompanies the invasion of colder air into a region previously covered by relatively warm or tropical air. Of course these phenomena had been experienced from time immemorial, but this is one of the very earliest and certainly one of the best accounts of the weather changes that occur at such

times. Later, when the structure of storms became more minutely understood, through the comparisons of observations simultaneously taken over many parts of the country, the sequence of phenomena here mentioned was implied by the phrase "passage of a line squall," since it is accompanied by squally conditions occurring simultaneously over a long line separating the two kinds of air, warm and cold. It now commonly is referred to as the passing of a "cold front," where the warm and relatively light air on the one side (eastern or southeastern) is pushed away by incoming air of lower temperature, greater density (at the ground pressure) and radically different direction (usually northwest).

While this paper contains no contribution to the understanding of the meteorological phenomena described, it is one of the best contributions we have to the facts upon which that understanding now is based.

"Account of the Formation of a Cumulus Cloud from the Action of a Fire." (Verbal comm.) *Proc. Amer. Philos. Soc.* **2**: 20, 116–117 (1841).

The first part of this paper constitutes one of the earliest accounts of the formation of a water-droplet cloud above a large fire, a phenomenon then, and even now, but little known and by many even denied. No attempt is made to explain the processes by which this cloud was formed, that is, through cooling of the water vapor in the ascending column to temperatures below the dew point, nor is any hint given of the fact that a considerable portion of this water vapor was furnished by the combustion of the material being burned. We must not, however, blame Mr. Bache for these omissions, since the chemical processes that furnished the water vapor were not at that time known, nor was the cooling of the air by expansion incident to ascent then understood or recognized.

Mr. Bache was a great-grandson of Benjamin Franklin, and grandson of Richard Bache, one of our first Postmaster Generals. He was of medium height and of pleasing countenance, indicating good nature and humor. He held various positions of distinction and honor, Professor at the University of Pennsylvania, President of Girard College and Director of the U. S. Coast and Geodetic Survey. He also had the rare distinction of going through the West Point of those days without a single demerit.

Espy, James P., "Deductions from Observations Made, and Facts Collected on the Path of the Brunswick Spout of June 19, 1835." *Trans. Amer. Philos. Soc.* **5**: 421–426 (1837).

This is one of the most accurate and all-inclusive descriptions we have of a tornado and its effects. There is little attempt, however, to give a scientific explanation of any of the numerous phenomena observed. Two surmises, still current, are tentatively offered, each noting a possible cause of the freakish noise which occurs in connection with tornadoes: one to the effect that it is owing to the beating of hail (common in tornadoes) against objects that have been carried up from the surface of the earth; the other, that this noise is somehow caused by electricity. Both explanations we now know to be quite erroneous. This is not meant to be a criticism of Espy's paper, the value of which lies in the extent and accuracy of its descriptions, which, of course, are essential to any scientific explanation of the origin and maintenance of such a storm.

"Law of Cooling of Atmospheric Air for Various Suddenly Diminished Pressures." (Abs.) *Proc. Amer. Philos. Soc.* **3**: 27, 155–157 (1843).

This is a digest of a communication submitted by Mr. Espy on a topic of fundamental importance to meteorology. It supports Espy's ideas of the cause of precipitation, and the genesis and maintenance of storms, as developed in his earlier work "The Theory of Storms." Espy's ideas in this connection we now know to have been entirely sound; at that time, however, they were radically new and not generally accepted.

Mr. Espy was of Huguenot origin, born in Pennsylvania, and educated mostly at Transylvania University, Lexington, Kentucky; he taught in schools at various places, including the Franklin Institute. From 1835 to the end of his life he devoted practically his entire time to the study of meteorological problems, to which subject he became one of America's greatest contributors. His work in this line was recognized by invitations to lecture before the British Association for the Advancement of Science and the French Academy of Sciences, and won for him also the coveted Magellanic Prize of the American Philosophical Society.

Loomis, Elias, "The Storm Which was Experienced Throughout the United States About the 20th of December, 1836." (Abs.) *Proc. Amer. Philos. Soc.* **1**: 11, 195–198 (1840); *Trans.*

Amer. Philos. Soc. **7**: 125–163 (1841); (Abs.) *Proc. Amer. Philos. Soc.* **2**: 22, 178 (1842).

This lengthy paper, an excellent abstract of which was given the year previously (that is, at the meeting of the Philosophical Society on April 17, 1840), is one of the most minute and exact accounts ever published of an extensive storm passing over the United States. The number of stations then keeping meteorological records was very small in comparison to the multitude in operation today, nevertheless they were sufficient to justify the broad conclusions, which we now know to be substantially accurate. Loomis considers several of the more important weather elements, that is, wind direction and strength, barometric pressure and its changes, temperature, and precipitation. He reaches important conclusions previously not generally accepted or, in some cases, not even suspected, that are entirely sound. He gives a minute and very accurate account of the phenomena that occurred during the passage of cold N.W. winds over a place that had just previously been covered by warm S.E., S. and S.W. winds; that is, he describes here the phenomena that occurred with the passage of what is now familiarly known as a cold front, and formerly in this country called a line squall.

He also undertakes to give an explanation of the causes of the barometric fluctuations that occur at the time of the passage of such a front. In this portion of his work he is least happy, since the explanations were not satisfactory to himself and are not acceptable today. However, we must not criticize the author too severely for his failure to furnish complete explanations of the barometric fluctuations, since we even now are not as sure of all of them in every particular as we should like to be. His accounts of how precipitation can be effected are most excellent, though, of course, concerning this phenomenon we now have more information than was available at that early date; and in consequence of this greater information of exactly what does occur we have been able to arrive at sounder explanations in some respects. The explanation that he gives of the cause of the rain along the line of minimum pressure, viz., the lifting of warm humid air on the one side of this line by inflowing colder air from the other side, and the consequent cooling of this warmer air, due to its decreasing pressure on being raised to higher elevations, would make one who reads this account believe, if he did not know to the contrary, that he was reading a sound theoretical discussion by one of our most advanced air mass analysts. This paper is a very important contribution to air mass analysis made more than one hundred years ago by one of our greatest contributors to the science of meteorology.

"Two Storms Which were Experienced throughout the United States, in the Month of February, 1842." (Abs.) *Proc. Amer. Philos. Soc.* **3**: 27, 50–56 (1843); *Trans. Amer. Philos. Soc.* **9**: 161–184 (1846).

This paper gives in tables and diagrams a remarkably detailed account of the weather phenomena over the United States during the passage of two very general storms. From these data, a number of conclusions are drawn concerning the temperature changes, pressure changes, cloudiness, precipitation, wind direction and velocity, and the direction and speed of travel of storms. This appears to be the most detailed and accurate account that had been made up to that date, and therefore a basis for safer conclusions than had before been possible. He concludes that the increase in temperature that precedes the most stormy portion of the disturbance, that is, precedes what we now call the cold front, is owing particularly to the importation of air from more southerly and therefore warmer regions, a conclusion that we now know to be entirely correct. He notes the possible contribution to increase in temperature from condensation, and from a blanketing of the earth by clouds, and correctly assumes that neither of these causes is the major factor. He again in this paper, as in his earlier one, attributes precipitation to the cooling of moist air incident to increase in height, maintaining, however, that this increase may be effected either by the blowing of the winds up a sloping surface, or by the air being raised to greater heights by being pushed up by the inflowing colder air—sound conclusions which meteorologists in general were a long time in accepting. Most of his other conclusions—of lesser importance, however, than those just mentioned—are substantially sound, except that his explanation of the origin of a general storm, inadequately supported at the time by observations and established physical principles. is, in the light of our present knowledge, far-fetched if not even fantastic. The causes he finds for the genesis of such storms are real, but wholly inadequate to produce the effects they are supposed to account for. Nevertheless, this paper, despite its minor faults, is indeed a meteorological classic.

Naturally enough, Mr. Loomis insists on the importance of the collection of meteorological data in sufficient abundance to permit charts, such as he had drawn for this storm, to be constructed for every half day of an entire year, and argues that with such weather maps before us our conclusions as to the causes of weather phenomena could not be in error and that the science of meteorology would then be both accurate and complete —optimistic conclusions it seems to us, now that we have several such charts, far more accurate than those he could get, for every day of every year, and yet about many particulars there still remain puzzling questions.

Mr. Loomis descended, along the paternal line, from a Connecticut colonial family. His scientific interest was, professionally, mathematics and astronomy, with meteorology as a side issue, an issue which, judging from the number of papers he contributed on it and their importance, must have absorbed more of his time and attention than did anything else, and which he promoted to a greater extent than any other subject. He taught in the Western Reserve University, the University of the City of New York, and finally Yale University. From his reputed success as a teacher and inspiring leader it seems that he must have been a man of great personal charm and wholesome influence.

Redfield, William C., "Observations on the Storm of December 15, 1839." *Trans. Amer. Philos. Soc.* **8**: 77–82 (1843).

This paper is a description of a West Indian hurricane which, in so far as it goes, is confirmatory of the fact that the winds in such hurricanes are largely circular about a common center, a fact which Redfield himself had previously established through the study of an earlier storm. The fact that these winds do rotate circularly was first established by Redfield, who also pointed out many important practical conclusions to be drawn therefrom.

Mr. Redfield was of a family large in number but poor in worldly goods. As a boy he was an apprentice to a saddle-maker. Later on when he began to get a slight start he became a merchant, and then, as a man of large means, the first considerable shipper of commerce and transporter of passengers up and down the Hudson River by steamboat. In 1848 he was elected the first President of the American Association for the Advancement of Science. His business interests and scientific acumen did not wholly perish with him, as he was the father of William Cox Redfield, Secretary of Commerce under President Wilson.

Chase, Pliny E., "Observations on Skylight Polarization at Philadelphia." *Proc. Amer. Philos. Soc.* **10**: 75, 196–197 (1866).

This paper is a detailed account of numerous careful observations made of sky polarization over an entire year. Its chief value is in its definite confirmation of conclusions that had already been reached by other observers; this phenomenon had been known and studied for more than half a century before the date of Chase's work, but for one reason or another, largely the adverse climatic conditions under which the observations had been made, needed further study.

Mr. Chase was born August 18, 1820, of an early New England colonial family of local influence and importance. For thirty-four years his father was Treasurer of Worcester County, Massachusetts; and his brother was president of Haverford College for ten years. During his boyhood and earlier life the straitened circumstances of his family saved him from being spoiled by too many indulgences, and gave him self-assurance through much hard work and little wasteful play. He was a brilliant student of Harvard College, and in later years became a worthy member of the Haverford faculty. His interests were very broad and his writings covered a wide field in both philosophy and science. One of his biographers says that he spoke six or seven different languages and read one hundred and twenty—some biographers are very generous!

AMERICAN GEOLOGY, 1850–1900

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(Read February 13, 1942, in Symposium on the Early History of Science and Learning in America)

Introduction

The following paper deals with the subject of studies in American geology during the latter half of the nineteenth century, the word studies being intended to cover the various phases of the work, from field observations to theoretical speculations. It deals with ideas rather than with persons, although the two are by their relation of parent and brain-child too intimately connected to be parted. The date, 1850, may seem to mark a beginning, but scientific thought does not begin, it evolves. Some reference to its earlier stages is necessary. And it happens that the American Philosophical Society had among its members two men of great distinction who contributed to geology in America in its infancy.

Benjamin Franklin was not a geologist. There were none in his day; those who saw Nature intelligently were called natural philosophers, and he was one. Referring to some earlier exchange of ideas he wrote the Abbé Soulavie in September, 1782, that he had merely remarked the occurrence of oyster shells in the stones of a certain rocky mountain in England, probably as high above the sea as the coal mines of Whitehaven were below it. This seemed to him proof that there had been "great bouleversement in the surface of that Island, some part of it having been depressed under the sea, and other parts which had been under it raised above it." These things seemed unlikely to happen, as he saw it, if the earth were solid to the center and he therefore "imagined that the internal part might be a fluid more dense and of greater specific gravity than any of the solids we are acquainted with, which therefore might swim in or upon that fluid. Thus the surface of the globe would be a shell, capable of being broken and disordered by any violent movements of the fluid upon which it rested." He then proceeded to speculate on the possibility that the internal fluid might be air which might be so compressed at a depth of some leagues as to be heavier than gold, and he reasons logically regarding the possible activities of such an interior gaseous body when heated. He goes on to the formation of such a globe and the arrangement of heavier and lighter constituents, to the movement of the parts to a common center, to the turning of the new globe upon its axis, and to the possibility that there might be a shifting of the axis, with consequent internal disturbances that might have produced the "confusion in which we find it." Verily he reasoned well, according to the knowledge of his day; had he known what we do now, he would have reasoned better than many now do.

Among the elements of our Colonial population there was a Scottish breed, often with an Irish strain, that was more at home in the wilderness than in the settlements. Men of that breed were the explorers. Born on the frontier they were versed in frontier lore, like the Indians whom they outwitted, and were accustomed to the solitude of the lone hunter in the wilderness. Not so William Maclure. Born in Ayr, Scotland, 1763, he did not come to this country until he was nineteen and then entered upon a business career. He was near forty when he traveled in Europe and absorbed geology in the school of Werner. But he became so fired with enthusiasm for that study that upon his return to America he "went forth with his hammer in his hand and his wallet on his shoulder" to traverse the valleys, mountains, and plateaus of eastern North America, from the Atlantic to the Great Lakes and the Mississippi. He is said to have crossed the Alleghanies more than fifty times, in that day in large part possible only on foot and often without even the log cabin of a pioneer for shelter. Only one who has trodden similar unexplored fastnesses of forest and mountain can appreciate the endurance, the indomitable purpose he demonstrated. And while thus exploring he made observations of such definite character of the kinds of rocks and their distribution that he was able to construct the first geological map of the eastern United States, 1809. He is appropriately known as the Founder of American Geology.

The following review takes up the story of geological research half a century later.

Rocks and Fossils

In 1850 the science of geology was in an early exploratory stage, especially in America. In England and Europe sufficient progress had been made in the study of the stratified rocks and the contained fossils to contrast markedly with American lack of knowledge. It could not have been otherwise. The first task of a geologist entering upon the geology of a new field is to observe, locate and study the distribution of the various rock formations he may discover. A map of the country, upon which he may delineate the results of his observations, is a prime requisite. But in 1850 the geography of America was scarcely more than sketched. Even the eastern country was known only in broad outlines and the West was just being explored. The making of maps suitable for geological purposes was not planned until the early eighties, by the U. S. Geological Survey, even though in saying this we have to recognize the excellence of the relatively small scale maps put out by the early Geological Surveys of New York, New Jersey, Pennsylvania, Virginia, and Tennessee. From the point of view of geological science they were reconnaissance maps, though excellent ones. Nevertheless, by 1850 material progress had been made in determining the ages and general distribution of the sedimentary rocks of the United States east of the Mississippi and of eastern Canada. Logan of Canada, Hall of New York, Cook of New Jersey, the Rogers brothers of Pennsylvania and Virginia, Safford of Tennessee, and many others who felt the urge to read the rocks had identified the strata of certain great periods of geological time, had classified them in order of relative age, and had mapped them with such accuracy as the conditions of surveying permitted. To do these men justice one must try to recall the conditions under which they searched out the facts. To step off of the widely separated beaten tracks was to step into the wilderness. In a short mile they were buried in the forest, without landmarks. Did you, motoring reader, ever sight your courses by pocket compass and measure your miles by paces; and then by the light of a fire or candle-dip plot your notes to construct the map upon which to lay out the observations you had made while subconsciously counting steps? They did. Theirs was the pioneer's, the hunter's life and joy, enriched by the satisfaction of discovering virgin facts. Theirs was a great opportunity and they pursued it with great success.

That they were able to accomplish so much was in part due to the fact that the great leaders in English geology, Sedgwick and Murchison, had established for that country a succession of strata and fossils, which was the same as that in eastern North America. It may not be assumed that the similarity was a matter of course, as we are prone to do. Had the Americans been studying the Pacific coast they would have found great dissimilarities. But it is a fact, though as yet unexplained, that the borders around each ocean basin have from remote ages been the scenes of similar, contemporaneous events, in some sequence peculiar to that basin. And thus it happened that Hall, the great paleontologist, and his colleagues were able to identify in America what might be called the North Atlantic succession of strata and fossils, which had been worked out in England by Sedgwick and Murchison.

It so happens that the North Atlantic sequence of strata contains a very complete representation of those fossils of marine organisms that record the evolution from an early stage to a fairly advanced one. The era thus became known as that of Ancient life, the Paleozoic. The strata were found to be divisible into certain groups, in both England and America. Each group could be identified, wherever found, by its characteristic fossils and each group received a name, chosen as that of a region in which the particular strata are typically represented. In Dana's *Manual* (2nd ed., 1863) may be found the subdivisions of the Paleozoic, established at that time; namely: Silurian, the Age of Mollusks, Devonian, the Age of Fishes, and Carboniferous, the Age of Coal or Coal Plants. Later, more refined studies have resulted in subdividing these three great ages and in readjusting their limits. It is beyond the scope of this paper, however, to consider the details of systematic classification. We may take space, nevertheless, to mention the recognition of the Cambrian or lowest part of Murchison's Silurian. Sedgwick identified a series of strata, which he regarded as distinct from the Silurian and proposed to call Cambrian. Murchison, and under his leadership the Geological Society of London, refused to consider Sedgwick's facts as evidence of another period and it remained for later geologists, among whom Charles D. Walcott was pre-eminent, to prove the correctness of Sedgwick's views.

Among the number of paleontologists who worked to establish the development of the Paleozoic system in North America, James Hall of New York (1811–1898) was the recognized, though oft-disputed authority. Authoritative statements and disputatious contradictions were characteristic of the times. James D. Dana, scholar and teacher, through his command of a more general, comprehensive knowledge of the science and through his facilities for publication, became a clearing house for the exposition of geology. His comprehensive *Manual* was a compendium of all branches from its first edition in 1866 to the last, the sixth, in 1897. As a college student in the late eighteen-seventies had I been asked why the earth was cooling, contracting, I might have answered: "Because Dana says so."

Dana had extraordinary mental grasp. He collected and collated the observations of his contemporaries with indefatigable industry. In these qualities he resembled Eduard Suess (1831–1914), the Master of European geologists at a slightly later date; and the resemblance went further. Both great thinkers possessed constructive imagination, both conceived original hypotheses of terrestrial scope, but neither was able to make a detached, impersonal estimate of the soundness of his theories. When a young man, Dana conceived the Contraction theory of mountain building; he taught it as long as he lived. Suess, convinced that no plateau could be raised against gravity, in his old age refused to consider the evidence that the plateaus of Asia had been so raised. Their opinions had become encysted.

The parallelism in geological development which characterized the opposite shores of the North Atlantic during Paleozoic time was continued during the succeeding Mesozoic and Tertiary eras. The similarity of strata of the Atlantic Coastal Plain with those of British and European sequences was observed by visiting geologists as far back as 1749 (P. Kalm) and 1777 (J. D. Schoepf), but the first attempt at correlation of Tertiary formations with the geological column then established abroad was made by William Maclure, a member of the American Philosophical Society. In 1809 he published in the *Transactions* of the Society certain observations on the geology of the United States in which he described the "Alluvial Formation," including under that designation all the evidently sedimentary strata of the Atlantic margin. More thorough studies, leading to the subdivision of the whole and the identification of the major divisions, were made during the decades immediately following by American geologists, among whom Vanuxem, Moore, and H. D. Rogers may be named, and in 1843 Sir Charles Lyell gave the weight of his authority to the transoceanic correlation.

Thus before 1850 the general outlines of geological history had been traced in eastern North America in comparison with those of Europe and the general principle that the history of the world was to be read in the sequences of strata and their contained fossils and could be expressed in terms of those eras, periods, and major subdivisions that had been established in Great Britain and western Europe, became fixed in geological thought.

We may pause a moment in the historical review of American geology to consider the basis of this general principle. It sprang from the observations of the English engineer, William Smith (1769–1839), who first observed a sequential relation of strata and the corresponding succession of fossil organisms. That fact remained without explanation long after it had become imbedded in geological science. It was recognized, indeed, that the greater part of the organisms had lived in the sea, but the diversity of habitats in the oceans and the dependence of any species upon limiting conditions were not soon appreciated. Consider, for instance, the parallelism of fossil successions in Great Britain and America; the real habitat of the marine organisms at any time after the boundaries of the North Atlantic were established was the warm current we call the Gulf Stream. Sweeping up the coast of North America and across to the British area it naturally carried identical forms of life throughout its course, which extended to the archipelago that then was Europe. The distribution of marine organisms the world over has in a similar manner followed the flow of ocean waters as directed by the lands during all biological time. But the shores of the lands have changed from age to age with the growth of continents and with the submergences of continental surfaces and the courses of migration of marine populations have thus been determined and diversified from epoch to epoch. The changes have been important factors in evolution. Therein lies the significance of the study of ancient geographies, that branch of the science known as Paleogeography, to which Dana made crude and tentative contributions. He did not, however, think in terms of evolution. We must remember that the

idea of creation by fiat governed all thinking, including geological, during the greater part of the nineteenth century.

Science still accepted the dictum of Cuvier (1769–1832):

> All things belonging to one of these (organic) forms, perpetuated since the beginnings of things, that is since the Creation, constitute what are species.

Dana, who recognized a plan of progress in life (*Manual,* 1863, page 600) wrote:

> The progress in climate and in the condition of air and waters involved a localization of tribes in time, chronographically, just as they are now localized by climate over the earth's surface, or geographically. Tribes were made for a special climate or condition of the globe; and when this climate or condition had passed in the earth's progress, the tribes no longer existed.

He closed the chapter on the plan of progress in the development of life with the following reverent words:

> Geology appears to bring us directly before the Creator; and while opening to us the methods through which the forces of Nature have accomplished His purpose it leads to no other solution of the great problem of creation than this: DEUS FECIT.

That creation might proceed according to a law of evolution was not then sensed.

The concept that a certain succession of living forms had been established by divine fiats for the whole world received confirmation as exploration extended over other lands than those in which fossil collecting had begun. In general it became possible to identify similar sequences of organic forms wherever the appropriate rocks occurred. Western North America was no exception. Geologists attached to the expeditions in search of a rail route to the Pacific coast found sufficient fossils to enable them to recognize the major divisions of the Paleozoic, Mesozoic, and Tertiary eras and so to lay the foundations for those studies of the stratigraphy and paleontology of the strata in the western plains and mountain ranges which have since been carried to a fairly complete knowledge of the history. The roster of geologists who contributed to that record is long and contains the names of the most distinguished: among the first pioneers were Marcou, Blake, Whitney, Newberry, and Hayden; King, Emmons, and Hague, Powell, Gilbert, and Dutton were also pioneers, but they continued their explorations into the period of organized geological surveys which culminated in the U. S. Geological Survey in 1879. Thereafter official surveys, either Federal or State, became the chief agencies for the advancement of geological knowledge of our country.

With this brief reference to the great achievement of establishing for America the standard geological time scale, on the basis of stratigraphy and paleontology, we must here turn to other branches of the science.

TIME AND EROSION

How old is the earth? That was a much discussed question during the latter half of the nineteenth century. At any earlier date the catastrophists could dismiss it with a reference to Genesis. J. D. Dana, himself, in 1863 (*Manual,* page 746) in comparing the geological and Biblical records of the Creation, interpreted the "day" of Genesis as the equivalent of an era of geological time and maintained that there could be no conflict between the two, since both were of one and the same divine origin. But estimates based upon the recession of Niagara Falls and upon the growth of coral reefs persuaded him that "Time is long . . . very long . . . , even when the earth was hastening on towards its last age." In this opinion he agreed with certain early estimates put forward on biological grounds. Darwin thought that evolution had required 200,000,000 years to reach the development of modern forms of life. Geophysicists, on the other hand, sternly narrowed the span. G. H. Darwin, on the assumption that the moon had separated from the earth, calculated the time since that event as 57,000,000 years. Lord Kelvin, reasoning from the postulate that the earth had cooled from a molten state, placed the period of that process since the formation of the earliest crust at not less than 20 nor more than 40 million years. His basic assumptions and conclusions were strongly challenged by T. C. Chamberlin, who realized the vast duration of geological ages. In conversation Chamberlin said: "Not forty millions, nor four hundred millions will suffice." This was in the year 1899. We now know that two thousand millions is a minimum, since granite nearly that old was intruded into still older rocks.

The method by which the age of a rock may now be determined with a degree of approximate accuracy never before dreamed of is based on the rate of degeneration of radio-active minerals and

was made possible by the researches of Madame Curie. It was of course not available to geologists between 1850 and 1900. They were, nevertheless, not without means of estimate. They had measured the thicknesses of strata and had observed their character. It was possible to approximate the rates of deposition of sands, muds, and calcareous oozes; and applying these rates to an observed thickness one could arrive at a figure that represented years. Schuchert (*Age of the Earth,* Nat. Research Council, 1931, page 18) gives a score of such estimates, which range in terms of millions of years from 3 to 1584 millions. Obviously, as was said by Arthur Holmes: "Most of these estimates are little more than rough guesses. We do not know how much of the story is lost to us, how much is hidden away."

Much of that which was lost to us, to use Holmes's phrase, is represented by blanks in the sedimentary record. The measurement of sediments could not take adequate account of the periods when little or no sediment was deposited, and those periods were very long. To grasp this relation we have to consider the processes and progress of erosion as elucidated by Powell and his eminent disciple, William M. Davis. Since the latter part of the eighteenth century the view of Hutton, a uniformitarian before the term was coined in geology, that erosion had always progressed as now, had governed speculation concerning its activities and effects. The operations of the processes were extremely gradual. By contrast the ferocity of erosion as displayed in the Grand Canyon of the Colorado almost shocked Powell. Such tremendous gorges, carved in so brief an epoch of geological time! He grasped the concept of recent uplift; he foresaw a slowing down of activity as the height should be worn away; he sensed the inevitable result, that the plateau must be reduced to a low surface, a uniform plain. Thus he came to recognize, what he afterwards always taught, that the process of erosion involves a cycle, comprising uplift (the action of terrestrial, internal forces), degradation (the effect of atmospheric activities), and sedimentation, including the transport and deposition of the waste. The process is one which runs down, unless renewed, and must end in a period of quiet, which endures until the subterranean forces awake again. During that period the land would remain reduced to its lowest possible level by erosion, to what Powell called "baselevel." His term was subsequently redefined as meaning a geometric surface, the lowest to which water could flow, and Davis's word "peneplain" or almost-plain came into general use.

Powell had philosophized regarding the processes of erosion. Davis studied its effects in detail. He dissected landscapes, distinguishing youthful, mature, and aged forms, and he thus set up the concepts of youth, maturity and old age as stages that might be recognized in canyons, valleys, hills, and plains. His work grew out of Powell's ideas during the last two decades of the century. He established a purely American branch of geology; Physiography he called it, but it is now known as Geomorphology. It provides a script by which he who runs may read the record of mountain growth. Mountains had been the symbol of the everlasting. They have come to be regarded as evidences of transitory activity of intermittent, subterranean forces. Since the most remote ages great uplifts have been eroded, have vanished from the scene; during vast ages great chains have been raised, one after another, without number. And the activity persists today. The Alps, the Himalayas, the Rockies, the Andes are all young. The grandest are among the youngest.

The writer was Davis's intimate contemporary, shared with him the opportunity and inspiration of contacts with Powell and Gilbert; but while Davis analyzed the sculptured forms of the landscape, I fixed my attention on the tools, on rivers, often the oldest features. They too can be eloquent. Yet I learned much from Davis and I would here record the debt I owe him. I recall an exquisite moment, when standing on a tower of the Great Wall of China, I read with his eyes the record of the uplift of the Tibetan heights fronting me and realized that I was the first among men who had passed that way who could decipher it.

It has indeed been inspiring to read the Appalachians, where we began, and also the mountains of Europe, Asia, Africa, South America, like an open book, in company with Davis's thought; but one inevitably follows Powell to the sources of that power which is so manifest in them. We get a new point of view. The action is recent, is even now at work. It is on a grand scale, as grand or grander than ever in the past. Can the force then be decadent? Is it running down? Is the earth a dying globe, "hastening on toward its last age" (Dana)? Thus the whole theory of terrestrial dynamics, as it was enter-

tained during the last century, as a deduction from the Laplacian idea of the origin of the solar system, was challenged by a better understanding of the surface features, which had always been open to investigation. That was the contribution of Powell, Gilbert, and Davis.

Mountains have long offered another series of problems, those relating to their architectural structure. They are essentially mechanical. Given a force, some force, competent to do the work, how could it act to push up such enormous masses and what did it do to the several blocks or masses of strata as it forced them up out of their original positions in the crust? The ideas involved are those of fracturing and folding on a grand scale. They come under the category of structural geology.

Structural Geology

The architecture of the Alps inspired the early students of that branch of geology which is called structural. The grandly majestic mountains may well seem awesome to many still, as they did to all men in the past; but to the daring few who began two centuries ago to seek to know them more intimately and to the many since, the Alps have revealed secrets which surpassed understanding. To the geologist who knows that normally beds of sandstone and limestone occur as flat sheets, in orderly sequences, they present the phenomenon of strata bent into huge folds without a break or strangely fractured and forced over one another in apparent confusion. The effects of enormous forces acting under abnormal conditions, stagger the imagination and defy analysis. The Alps present one challenge of structural geology, which no one has yet been able successfully to meet.

Two great observers labored in the Alps during the first half of the nineteenth century and lived on into the second half. They were Bernard Studer (1794–1887) and Arnold Escher von der Linth (1807–1872). Apart from the great volume of facts which they observed and recorded painstakingly as they saw them, they are deserving of profound respect for their outstanding selflessness. They investigated, studied, pondered, in harmony with one another, without controversy, and they gave the fruits of their years unreservedly to all who sought them. That is the testimony of their equally great pupil and successor, Albert Heim. Heim was the first to reason out the principle which is now a truism, that any bed of rock, however strong and rigid, may be bent without breaking, provided that it be so loaded that it cannot separate. He also saw that solid rock had flowed under adequate pressure, where adequately confined, and that it had recrystallized during such movements. In his beautiful volume, *Das Mechanismus der Gebirgsbildung,* 1878, he opened the way to understanding the changes of form and relations that are forced upon masses of rock in the movements of rising mountains. It is one of the tragedies of geological science that his lead has not been followed by European geologists. He was silenced in 1903, at the Vienna Geological Congress, by the oratory of Lugeon, whose brilliant imagination had conceived an explanation of Alpine structure that has proved convincing to many, though it contradicts the laws of mechanics. Perhaps one reason for this psychological phenomenon is that the structure of the Alps is so intricate as to render analysis almost impossible. To follow Heim and apply the laws of mechanics to the interpretation of mountain structures we have to study ranges where the effects are simpler.

In America the belt of the Appalachian mountains presents an unusually simple system of long, narrow folds. The folded rocks are bedded sandstone, shales, and limestones, which were piled up to thicknesses exceeding ten thousand feet. They were spread in flat sheets. They are now bent into arches and troughs, in alternation, like wrinkled paper. The brothers, H. D. and W. B. Rogers, executing the geological surveys of Pennsylvania and Virginia during two decades before 1850, observed the folding. They had no bird's-eye view of it. But tramping across Brushy Mountain, for example, they saw that beds of sandstone which formed the northeastern ridge reappeared in the northwestern ridge and were continuous under the central mass. At Hancock on the Potomac they saw, no doubt, the beautiful arch of limestone. Observations such as these multiplied till it became clear that the structure was one of successive folds, in a belt that varies from 60 to 100 miles or more across. The effect is that which would be produced if the belt had been narrowed by compression, but the Rogers brothers thought it impossible that simple compression could have so acted. They conceived a compound action, a combination of vertical and horizontal forces, like rising and falling waves; and they found a not unreasonable cause in the agitation of a thin crust, floating upon a molten interior. Their inferences were consistent with the then ruling ideas of a

molten globe and of catastrophic activity of terrestrial forces. Unfortunately for the theory the assumption of a thin crust floating on a fluid interior was mistaken, as was subsequently demonstrated by the geophysicists. Kelvin in 1862 calculated the effect of tidal attractions and showed that the earth must be as rigid as steel to resist them, as it does.

More detailed studies of the Appalachian folds and related structures continued to be made by individuals and State surveys until the entire mountain system had been mapped, from Canada to Alabama. The U. S. Geological Survey established the Appalachian division in 1885, under G. K. Gilbert, whose assistant, Willis, made a series of experiments to test the application of mechanical principles to folding, as elucidated by Heim. Appalachian folding became a classical concept in structural geology, a type of structure, thought to be characteristic of all mountain ranges. This is not true. Gilbert in course of his reconnaissance surveys in Nevada during the eighteen-seventies observed that the long, narrow ranges which rise from the flat desert valleys of the Great Basin, are not arches or anticlines of a series of folds, but are blocks cut out by longitudinal faults and pushed up vertically. Subsequent studies have amply confirmed his views, although the mechanism of uplift has not been fully explained. There are various other mountain ranges, concerning which we now know that they differ in structure from the Appalachians, such as the Rockies or the Coast ranges of California, in both of which wedges replace folds as raised segments. But at the close of the nineteenth century little progress had been made in the mechanics of rock deformation, as this phase of structural geology is called. The concept of a folded crust prevailed and governed the majority opinion regarding the nature of mountain "folds." It strongly influenced speculation concerning the causes.

Theories of Mountain Building

Before we can do justice to the ingenuity with which the problems of mountains and their uplift were attacked during the nineteenth century we need to consider the ideas of terrestrial dynamics that prevailed, and which, indeed, are by no means obsolete today.

It was not doubted that the Laplacian theory of the origin of the solar system was essentially correct and its corollary that the earth had cooled from a molten state was a basic assumption. Dana wrote (*Manual,* 1863, page 739):

> The earth in igneous fusion had no more distinction of parts than a germ. Afterwards the continents, while still beneath the waters, began to take shape. Then as the seas deepened, the first dry land appeared, low barren, and lifeless. Under slow intestine movements and the concurrent action of the enveloping waters, the dry land expanded, strata formed, and as these processes went on, mountains by degrees rose, each in its appointed place.

The Contraction theory of mountain building, which Dana attributed to Newton, but which is identified with him, himself, is the hypothesis which was most favorably considered during the half century with which we are dealing; it is still preferred by some geologists. It rests upon the assumption that the globe is cooling and therefore shrinking bodily; and as the body shrinks it leaves the crust unsupported until the weight of the latter causes it to collapse. In collapsing the flattening arc pushes adjacent segments aside. Dana supported the theory from the data of his first publication on the subject in 1847 to his death in 1897. On the other hand it was strongly attacked by such investigators as Dutton and Van Hise on the ground that any possible contraction of the globe was quite inadequate to account for the observed shortening of the crust. This argument became very cogent when it was realized that the shortening of the Alpine zone, for instance, amounted to at least 72 miles and had been produced during the relatively recent, Tertiary period. It was exceedingly difficult to imagine that the globe could have shrunk sufficiently, in so short a time, so to reduce its circumference; and this objection gained much weight as knowledge of other young mountain systems increased. The close of the century found the Contraction theory still a subject of debate, with opinion running against it with such students as Dutton, Van Hise, and Chamberlin.

Among the contemporaries of the Contraction theory was the Gravitation theory of James Hall. That paleontologist had observed that the Paleozoic strata of eastern North America had been compressed and folded where they were thick, *i.e.,* in the Appalachian belt, whereas they had remained undisturbed where they were thin, as in the upper Mississippi valley. He reasoned that the folding had been occasioned by the curvature of the strata as they subsided, the result being that the lower beds were stretched, while the upper ones were compressed. Dana combated the the-

ory on the ground that the curvature could not supply either the pressure or the amount of compression required by the facts. Hall, however, never yielded the point and republished in 1882 his original statement of 1857.

One phase of the Gravitation theory remains debatable even today, since it involves the unknown factor, the capacity of the earth's crust to bear loading. Where great thicknesses of sediment have accumulated gradually in a trough that has deepened as gradually, did the loading occasion the subsidence? Or did subsidence, due to some other cause, provide depth for the loading? The answer to this question involves the strength or rigidity of the foundation rocks on which the crust rests. If they are strong and rigid something more than the superficial load of a few thousand feet of sediment would be required to displace them. On the other hand, if they are fluid or weakly plastic they may gradually flow away from under the load as it accumulates. The crust would, in the latter case, either be in balance or be rising or sinking to restore a balance, after some change had disturbed the normal condition.

Dutton, in 1884, crystallized the idea of normal equilibrium of the crust in the term *isostasy,* meaning equal balance, and reasoned that the normal condition was disturbed by erosion of a plateau or mountain range and the transfer of its mass to another area. We shall presently have to consider the theory of isostasy, but just at present we are concerned with its possible significance as a cause of mountain building.

Dutton and Powell had worked together in the exploration of the High Plateaus of Utah and both observed that they had been elevated; but they reached different inferences from the fact. Powell concluded that the initiative lay with forces of the interior of the earth, which in some way raised the surface and started the process of erosion. When that had run down by reducing the land to a featureless level there would be no further activity until the internal forces renewed the uplift. Dutton, per contra, thought that the transfer of load, due to erosion and sedimentation, constituted a "continuous disturbance of isostasy." Weighing the relation of the intensity of the disturbing forces against the rigidity or viscosity of deeply buried rocks he inclined to the opinion that the former would be competent to produce flow and that there must be a movement of material from the loaded trough toward the unloaded mountain zone. Thus the loaded sea bottom would be pushed toward the unloaded land, horizontally. This, in his judgment, would supply precisely the kind of force required to explain the origin of systematic plications.

Heat has always been recognized as a source of energy, which in some way had to do with mountain building, but it was an obscure agency of uncertain potency and unknown habitat. The brothers Rogers had imagined it at the temperature of melting, close beneath a thin crust; Kelvin had banished it to greater depths. Mellard Reade, however, put it to work. He followed Hall in attributing subsidence of a loaded trough to the load and reasoned that the depressed strata must become heated as they sank into the zone of higher temperature. They would consequently expand and become compressed and folded. As it appeared doubtful that sufficient rise of temperature would thus be realized Reade called in that to be derived from intrusives, but even so, as was argued by Dana, the amount of expansion presumably fell far short of that which would be necessary to account for the measured effects of compression.

Neither gravity nor any possible rise of temperature in more or less deeply buried rocks seemed competent to furnish energy sufficient to push up mountain chains. The theories of Dana, Hall, and Dutton assumed the competency of gravity. That of Reade rested on heat. They were all dynamically too weak. At the close of the century the leading American geologists were agnostics on the subject of mountain building. The contraction theory, however, still commanded respectful consideration and Dutton's hypothesis of isostatic adjustment was considered at least a real, though probably inadequate condition.

Isostasy, the Problem of Balance

As Dutton pointed out in 1884 in his paper on "Some of the Greater Problems of Physical Geology," the earth, if it were homogeneous, would present a smooth surface and be covered by a universal ocean. Since there are oceanic basins and protuberant lands it could be homogeneous only if strong enough to support the weight of the higher segments as against that of the lower. But in large masses rocks are not strong enough; hence it follows that the high-standing masses, the continents, must be of lighter material than the low-lying masses, the ocean beds. This is now a well established fact, but in Dutton's day it was no more than a sound inference, which was discussed by Dutton and Gilbert when they relaxed

in the Cosmos Club. Dutton was inclined to emphasize the importance of observations of the intensity of gravity, as a means of ascertaining the approach to a balanced condition of lowlands and highlands. They were made by the U. S. Coast and Geodetic Survey with extraordinary thoroughness and with a degree of detail that may well be described as astronomical. The names of John Hayford and William Bowie are indelibly linked with that prolonged investigation. Many hundreds of observations for the attraction of gravity were made and compared by an elaborate system of mathematical adjustments. The condition of perfect isostasy was assumed and those adjusted values of gravity which were most nearly in accord were regarded as most nearly correct. Many departures from the accepted normal value were found, indicating some degree of rigidity of the crust, but it appeared to be slight, and the condition of equilibrium almost complete. Time has not brought a more definite estimate. An approach to equilibrium certainly characterizes the condition of support of the earth's crust, but rigidity carries a part of the load, an unknown and possibly variable part.

Gilbert went directly to the question: What is the largest mass that can be supported by the rigidity of the crust? He himself furnished the nearest approach to a correct estimate through studies of the shorelines of extinct Lake Bonneville, the greater ancestor of Great Salt Lake. The beaches are no longer level. They have been domed up, as if in response to the removal of the load of water as it evaporated. For comparison Gilbert estimated the masses of isolated mountains and other features which appear to stand upon the crust and to be borne as loads by virtue of its rigidity. He arrived at a working hypothesis, which he stated as follows:

> Mountains, mountain ranges, and valleys of magnitude equivalent to mountains exist generally in virtue of the rigidity of the earth's crust; continents, plateaus, and oceanic basins exist in virtue of isostatic equilibrium in a crust heterogeneous as to density.

Subsequent research confirmed this estimate and it is widely accepted as a working hypothesis, as Gilbert put it. On the other hand there are many geologists who accept the conclusion that isostasy is nearly perfect. A factor which is now known to affect that condition is the area over which a uniform load extends. The greater the diameter of the loaded area, the deeper the penetration of the weight in the elastic crust and the weaker the support. The point had not escaped Gilbert's penetrating analysis. In a discussion of the strength of the earth's crust, in 1890, he answered a question put by Chamberlin as to the area of an effective load by saying frankly:

> It seems clear to me that the imposition of a long narrow ridge will be no more effective in producing deformation than a small portion of the same ridge, but it is not clear whether a broad lens of added matter will be as effective as a more compact lens of the same weight.

The true relation was eventually discovered by engineering experiments and experience of foundations, but not till long years afterwards.

Minerals and Crystals

Mineralogy, the oldest branch of geology, received a great deal of attention in America during the nineteenth century. In this as in broader fields James D. Dana was a leader. He made a specialty of the description and classification of minerals and was for sixty years an American authority whose *Manual of Mineralogy* was standard. The first edition (1837) was a relatively modest work, in which mineral species were described according to a "Natural Classification," under names "constructed according to the plan so advantageously pursued in Botany and Zoology." This system underwent successive changes toward a "systematic nomenclature" in the second (1844), the third (1850), and fourth (1855) editions. Finally in the fifth (1868) it was developed into a "general system," "based on a comprehensive view of the characters of minerals as species in the inorganic kingdom of nature, the pre-eminence being given to chemical, the next place to crystallographic, the third to different physical characters."

These advances toward a system in classification marked the recognition of the fact that minerals, as chemical compounds, are subject to the laws of chemistry and are capable of chemical reactions to changes of environment. Though it was a gradual change in the point of view, it was a very profound one, full of significance for the future of mineralogy. What had been a purely descriptive study, of interest chiefly to collectors, became analytical research, directed toward better understanding of the nature of minerals and crystals. Dead minerals came to life.

In the sixth edition of *Dana's Mineralogy*, Edward S. Dana referred to the great advances made

in chemistry and to the importance of developments in optical investigation, leading to new views regarding the molecular structure of crystals. Crystals had long been examined under the microscope, especially with reference to what could be seen in the thin edges of chips, but it was not until the eighteen-seventies that the art of grinding thin, translucent sections was developed successfully. It became the basis of that study which is now known as Petrography. An extremely fertile field, richly sown with facts relating to the characteristics of minerals and their family connections, was opened to examination. The harvest was almost embarrassingly abundant. Hosts of new species appeared. Naming became the fashion. The resulting confusion extended to the description and classification of rocks. Late in the century a group of petrographers, consisting of Whitman Cross, Joseph Iddings, L. V. Pirsson, and Henry Washington, undertook to frame a quantitative classification of igneous rocks, based on chemical and quantitative, mineralogical characters. The basic postulate was that the nature of the rock was determined by the chemical composition of the melt from which the minerals had crystallized. The research therefore directed attention toward the study of molten bodies, magmas, beneath the outer crust. It was not published, however, until 1903.

Petrography is a descriptive science; the related study, which investigates the genesis of rocks, is called Petrology. Its dynamic nature may be indicated by a quotation from Rosenbusch, founder of Petrography, who wrote:

> A rock becomes alive to me only when I have grasped its relations to our planet and its history (1876).

He glimpsed a future in which, through study of rocks in the making, we may approach an understanding of the processes that are active in the mass of the earth, under the great pressures, at the high temperatures, through the enormous forces therein existing. In so doing he was at least half a century ahead of others. The recognition of Petrology as a promising branch of geology was delayed till about 1890 and its development has been slow. It could not be otherwise. Physics, which must lead in Dynamics, was stopped at the atom. It was impossible to penetrate the realm of subterranean forces until that citadel was reduced.

Metamorphism

Dana in 1866 (*Manual of Geology*) defined the alteration of rocks which is known as metamorphism as that process of change which has affected many strata of the globe after their original deposition. He added that the term is applied especially when the changes have affected great series of strata, producing as an extreme result a crystallization of the rocks. The essence of the definition lay in the time relation and in the magnitude of the altered mass. Sir Archibald Geikie in 1893 (*Text-book of Geology*) wrote a similar definition: "Metamorphism is a crystalline (usualiy also a chemical) rearrangement of the constituents of a rock." Both definitions are purely descriptive; the idea of a cause does not enter. Later investigators, drawing inferences as to the dominant condition involved in causing the change, distinguished thermo-metamorphism, dynamic metamorphism, regional metamorphism, etc., according to the circumstances.

With the development of optical methods of study and especially of the examination of thin sections under the microscope, the investigation of metamorphic rocks was pursued as analytical research, having for its aim an understanding of the effects of heat, confining pressure, and an unbalanced pressure during the process of recrystallization. The microscope confirmed what had long been known on a larger scale, namely that certain laminated or fibrous rocks, the gneisses and schists, had been forcibly elongated and their minerals rearranged in parallelism, partly by rotation of the original crystals, partly by the growth of new ones. The effects were in part mechanical, in part physico-chemical, in varying degrees, and involved shearing of an exceedingly intimate character, and also flow of the solid rock under extreme conditions. Prominent among the many geologists who pursued these difficult studies were the Finnish geologist, Sederholm, and C. R. Van Hise of Wisconsin. The latter, as a result of investigations begun with R. D. Irving in 1882, in the Lake Superior iron ore region, published in 1904 a very comprehensive treatise on metamorphism, 1200 pages in length and designed to cover the entire subject of rock alteration. In the then state of knowledge of physics and petrology it was an exhaustive treatise, but in consequence of the revolution in atomic physics and its bearing on problems relating to crystallization from magmas and recrystallization of solid rocks it has now become a reference work, of classical interest

chiefly. In that respect it has shared the fate of nearly all the compendiums of geological learning of the last century.

In the study of historical geology a knowledge of metamorphism has made it possible to read the records of that remoter past, which is represented by the so-called Archean rocks. The vast areas of these crystallines in North America and Europe, which were known to Dana, were described by him as continental nuclei, as masses formed at that stage of the earth's development when continents began to grow, possibly before there were any sedimentary rocks. But through researches in metamorphism it became possible to distinguish metamorphosed sediments from rocks of igneous origin and to determine the sequence of their relations. Gradually the ancient record has been read, not only in America, but in all continents, and much is now known of the geography and terrestrial activities of those long past eras. The imaginary stages of earth history, the creation of lands and seas, the catastrophies from which they sprang or in which they, Atlantis-like, disappeared, were found to be mythical as the gods of Scandinavia and were replaced by the normal events of terrestrial change.

Thus the study of the metamorphosed rocks widens our understanding in two directions. It throws a beam of light into vistas of the past of which we never dreamed and it uncovers the processes of gradual change in the realms of force far beneath our feet.

Legacy of the Nineteenth Century

The nineteenth century bequeathed to Geology a rich inheritance of facts. In every branch, in historical, paleontological, structural, mineralogical, economic, in every subject the mass of observations was so great that no scholar could embrace it all. Leaders emerged as specialists, not like Dana as all-round geologists. Special societies were contemplated and eventually organized in order that students of one branch or another might have opportunity to discuss with fellows who would understand, rather than to talk to experts in other fields who could not be expected to. And yet an individual advancing in his preferred line of research often found need of support from a colleague or from a group of colleagues proceeding on other lines, and mutual exchange of ideas characterized the relations of the truly broad-minded. The open mind was encouraged by such outstanding scientists as Chamberlin and Gilbert, authors of the method of multiple hypotheses, and co-operation was vigorously promoted by the Geological Society of America, the U. S. Geological Survey, and the geological faculties of Harvard, Yale, Wisconsin, Chicago, and other leading universities. American geology entered the twentieth century strongly manned and rich in material for study. The condition was an exceedingly healthy one, in marked contrast to that which had prevailed when paucity of information and tenacity of individual opinion retarded progress.

Nevertheless, the science was very weak in theory. Chamberlin when asked on one occasion what occupied his thought replied in miner's phrase: "Prospecting old drifts to see what, if anything, of value is left in them." He referred specifically to the Laplacian hypothesis of the genesis of the solar system (which he and Moulton proved fallacious) and its consequences in geology, the once molten, cooling globe; the once steaming, now congenial atmosphere, the once potent, now dying earth. These ideas had become suspect. Advanced students entertained the thought that the planet was much older than had been estimated, that its vast energies were still very effective, and that the changes they had wrought in its crust during the later geological periods were so great that they could not have been worked by gravity and residual heat. Yet no other forces were known and the framing of new hypotheses to replace the old proceeded doubtfully. In this day, 1942, it is known that those doubts were fully justified; however, thanks to Madame Curie the inexhaustible energies of the atoms of the globe, that is to say the energies of its entire mass, are potentially available to geological speculation. It is no longer a question of power, but one of its distribution and of the mechanisms through which heat, gravity, and atomic forces work.

ENGINEERING IN OUR EARLY HISTORY

(The American Philosophical Society and Engineering from 1768 to 1876)

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(*Read February 13, 1942, in Symposium on the Early History of Science and Learning in America*)

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WHEN an intelligent layman makes a general reference to engineering, he usually visualizes spectacular physical entities. For illustration, he thinks of steam engines, locomotives, steamships, electric power-generating plants and transmission facilities, automobiles, railroads and hard-surfaced highways, radio equipment as well as telegraph and telephone systems, and great manufacturing projects. This is a narrow view. Such objects are only physical symbols of engineering. The real engineering is an intellectual phenomenon out of which these and an infinitude of other physical symbols come to useful birth. This true engineering is a process rather than a physical result—the process of planning, organizing and executing works intended for identifying and directing the forces of nature to man's service. Emphasis rests on economic service, but the connotations cover a multitude of collaterals among what are often spoken of as "intangibles."

There is a real contrast between present conditions and those of one hundred and seventy-five years back with respect to engineering, because of the extraordinary differences in the accumulated knowledge at the two ends of the period and the clearer apprehension now of methods for making knowledge useful to the human being. This brings us squarely in front of the query—Why did the men of great intellect, great human sympathies, great wisdom and worldly judgment, and great world experience come to put so much emphasis on "promoting useful knowledge" when in 1768 they were establishing the American Philosophical Society by merging two smaller societies, in both of which smaller societies Benjamin Franklin was interested?

These men included Franklin (the first President of the American Philosophical Society), Dr. David Rittenhouse (the second President of the Society), Thomas Jefferson (who was the third President, although not a member in the first group of members). They were saturated with interest in the extension of knowledge, and in the welfare of the people of the young nation and their education. The moderate number of other members originally composing the Society were fellow spirits. Each such man possesses "inspired curiosity," whatever vocation in life has been determined for him by tastes or circumstances; and he knows that the results of cultivating that curiosity will contribute to the welfare of humanity.

I will try to answer the query earlier noted of why these men put so much emphasis on "promoting useful knowledge":

To begin with, from some knowledge of the lives of a number of these men, I have ventured to infer that the wise group of men who composed early membership in the society had the vision that no knowledge which is truly *knowledge* is useless—that is, that it all ultimately may be useful by virtue of being made contributory to the comfort, convenience, security and happiness of the nation's people, and therefore that all knowledge is worthy of being "promoted." Also, that the earlier the expounded knowledge is brought into personal service of people, the heartier is its contribution to civilization.

This theorem relates, you will observe, to the whole field of learning and it suggests no circumscribing of the individual fields of action for the many gifted individuals of many tastes who wish to cultivate intensely specific aspects of learning. The first volume of the Society's *Transactions* expounds astronomy, culture of plants, transportation, simple medicine, natural history—and all apparently with equal emphasis. Medicine and surgery then had not achieved their standing in science, and the applications in the minds of these men appear to have been mostly in the broad expanses of agriculture (animal husbandry, arboriculture, viniculture, cropping) and what we now call engineering.

The three great human phenomena—life in communities, engineering, and civilization—have de-

veloped with each other hand in hand, as a part of improved conditions of living for man, through the whole period for which we have permanent records. A like relation for the three also is indicated by artifacts and other remains to have germinated slowly in prehistory. The intimacies and the mutual helpfulnesses of the three sets of phenomena have experienced (and continue to experience) ups and downs, but these intimacies and mutual helpfulnesses have grown in intensity over thousands of years (and continue to grow in intensity) as sound, essential features in the human life of a crowded world. Measured in terms of the aggregate of comfort, happiness and mutual sympathies affecting populations, the improvement is slow. It gives the impression of alternate progressions and retrogressions in various parts of the world, but the measure on the whole is favorable as time goes forward and education expands.

Let us see what we mean by the three phenomena denoted by the words civilization, engineering and community living:

Civilization connotes an advanced state of material and social well-being among men, with which is associated a sympathetic mutual interest between individuals and by the individuals for community welfare. It develops only where considerable numbers of men gather together for mutually desirable ends which provide a degree of unity in feelings, in beliefs and in motives. Through it there develops a sense of comity between men who are living in communities—that is, living in geographical nearness to each other so that they must give concern to their mutual relations—and this leads to developing ethical and moral relationships among men. These qualities of ethical and moral character compose a substance of civilization, but they have blossomed to fuller degree as firmer integration of communities has followed in the wake of engineering inventions that contribute to security of living and the increase of comfort, convenience and leisure above the conditions of a bare subsistence.

We now are far along in the process of causing financial capital, with its products in the way of machines, to assume the major physical burden of industrial production and to lift that burden from the muscles of individual men; by which process we have so increased industrial output per man that daily hours of labor have been incredibly shortened and daily pay has been increased. The comfort of the lowliest families now surpasses that of families of middle levels a few centuries ago. This process continues to accelerate; but our results are often misused by demagogues, and we fumble in the clearness of our efforts.

It is reasonable to propose to you that rectification of the situation challenges earnest study carried on in mutual coöperation by engineers, historians, economists, social scientists and psychologists. It is also pertinent to note that the wise founders of the American Philosophical Society seem to have assumed mutual coöperation by learned men in various branches, wherever cooperation may be indicated and the learned individuals are attracted by common interests.

The outcome of increased convenience, comfort, safety to life, and security of living for men, and the lifting of heavy labor from men's muscles, all of which ought to contribute to the health and happiness of populations and to their ethical and moral attitudes, is plain to everyone who will try to think clearly. Men already had moved across the threshold of intellectual accomplishments partaking of the character of simple engineering before the historical era with its written records began. The long series of records within the historical era show that intelligence secured a slowly increasing influence on man's affairs as time progressed. The ideals of civilization slowly expanded under this influence, and mankind seems to have moved falteringly (and perhaps unconsciously) toward the goal of finding available for each individual a joy in accomplishment within ethical bounds. The excitement arising from such accomplishments may become ultimately as influential in attracting votaries as the physical excitement heretofore secured from participating in disorder and warfare. Progress is slow but the goal some day may be reached, provided improved guidance is bestowed on man.

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The foregoing gives the background and the anticipations with respect to engineering, as they might have been viewed in 1768. The American Philosophical Society built on these as well as on the backgrounds and anticipations in other fields of knowledge.

Now let us turn to consider the men who were active in improvements of an engineering nature, about which they contributed information to the public through this Society during the first eleven decades of the Society's being—that is, until about the date of that incident of industrial significance in this country which we call the Centennial Ex-

position (1876). That is an important date because of the influence which the "Centennial" exerted in arousing interest in and accelerating industrial growth in this country. After outlining the personal qualities of a few of the men of the Society who were interested in the industrial aspects of the nation's life, I will then deal with a few of their achievements.

Benjamin Franklin (1706–1790), printer, business man, humorist, diplomatist, scientist and engineer, was probably the most influential member of the Society within that period in sowing active engineering thoughts in the minds of the public.

Franklin's active mind was so keen that there was scarcely a conception or an object which inconvenienced him that he did not subject to experiment and improve by invention. In the field of engineering he touched heating and ventilating, transportation by water, and other subjects equally diverse. He was under obligation at one short time to serve as military engineer when performing some official functions in eastern Pennsylvania, and built fortifications on the Indian frontier of his area. He successfully accomplished the task, although it was not to his taste. Franklin and his activities were so strongly in the public mind that his interest in the American Philosophical Society lent an influence on the public regarding engineering, as well as other matters, which outran the merit of his own publications in this field.

Franklin's popular influence was so all pervasive and strong that I think we must ascribe to him first place among those who have contributed engineering knowledge to the public through this Society, although his Society publications within the field are by no means as intrinsically important as papers of some others. His was an indirect manner of contributing serviceably through the Society, but his contributions nevertheless were very real.

Of the some sixty other individual contributors of engineering topics to the *Transactions* and *Proceedings* prior to 1876, I think only one (Elihu Thomson) vied with Franklin in lifting the influence of the Society in engineering matters; but many others left marked individual stamps on the engineering practices of the country and contributed to the influence of the Society through their published papers. In passing, it may be assumed that members, however able and creative in the engineering industries, who are not represented in the Society's publications although they are in the Society's roster of membership, such as Robert Fulton, several eminent du Ponts, S. F. B. Morse, Eckley B. Coxe, John Ericsson, have not contributed to public knowledge through the Society. They are among those whom we gladly count great but whose services to the public are through other channels.

I will speak regarding the personal characteristics of a few of the Society members besides Franklin, somewhat to represent the scope of engineering supported by the Society.

John Lukens (1720–1789), Surveyor-General of Pennsylvania and Delaware in 1761 to 1776 and later of Pennsylvania, was among the original members of the Society. He appears to have taken considerable interest in its affairs, although his name is entered but once in the classified index of the Society's publications. As his official title leads one to infer, he had a primary interest in the geography of Pennsylvania and adjacent Colonies. In the first year of the Society's life he led a committee appointed by the Society to seek a route for transportation between the Susquehanna and Delaware Rivers, and he made an important report on canals and highways that would fulfill the needs.

In 1761 Lukens and others were appointed to determine and mark the lines of boundary between Pennsylvania, Maryland, and Delaware as fixed by the royal grants and proprietory agreements. This commission was put under way with inadequate instruments. Slow progress annoyed the Proprietors, and two "surveyors and mathematicians" (Charles Mason and Jeremiah Dixon) with better instruments were sent from England in 1763 to take up the work. Having verified the antecedent work, they continued the line westward to near its western terminus when they were stopped by Indians. From them the boundary between Pennsylvania and Maryland received its name—Mason and Dixon Line. The continuation of the line to its westward terminus and marking the meridian of longitude which is the western boundary of Pennsylvania was authorized by Virginia and Pennsylvania in 1781. Then again John Lukens was appointed with one other for the duty. David Rittenhouse was a commissioner for the State of Pennsylvania. The esteem in which this work was held is indicated by the fact that both Mason and Dixon were elected to membership in the American Philosophical Society, Mason in 1767 and Dixon in 1768. Their temporary observatory, erected for determining the latitude of

Philadelphia, is reported to have been the first astronomical observatory on the continent. Dr. Rittenhouse's observatory, begun in 1768, is reported to have been the first permanent one.

Benjamin H. Latrobe (1764–1820), architect and engineer, is an esteemed example of an Englishman welcomed to America. Born in England, he spent part of his boyhood in Germany, but he was back in England at the age of twenty-two. He studied architecture under a master in the Greek revival, studied engineering under the great English civil engineer, John Smeaton, and superintended some canal work. He came to America at the age of thirty-three and immediately plunged into architecture and civil engineering. The *Dictionary of American Biography* says of him: "He did much to improve architecture and make it a profession rather than a polite accomplishment of gentlemen amateurs. Apprenticeship in his office constituted the first important professional training in engineering and architecture in America. It was his pupils who built the first American railroads; his style continued to dominate American building down to the Civil War." Busy professional man as he was, he nevertheless in the ten years from 1799 to 1809 contributed six articles (two of them biological in nature) to the publications of the Society.

Moncure Robinson (1802–1891), born in Richmond, Virginia, was interested as a boy in rural phenomena. Applying for employment on the topographical survey just authorized to be run from Richmond to the Ohio River, he was put aside on the ground of his youth. Nevertheless he volunteered and accompanied the survey party without compensation. A second trip in similar territory was made to examine wild lands, and employment in canal construction was followed by three years of study and observation in Europe. By the time of his return to this country (1828) he had transferred his interest from transportation by canal to the then new transportation by rail, and he thereafter confined his professional activities to railroads until he retired. His work in planning and constructing the Philadelphia and Reading Railroad was of a highly distinguished character and was stamped with an unusual degree of originality and foresight. Robinson was a man of resourcefulness and initiative and was widely called on for advice. In 1853 he was elected an honorary member of the American Society of Civil Engineers. He made one contribution to the publications of the American Philosophical Society—namely, the transmission of a report on the then proposed railroad tunnel between Calais, France, and Dover, England.

Elihu Thomson (1853–1937) most of us have known personally. His keen observation, lively imagination and strong powers of invention joined to make him a great experimenter, and a preponderant interest in the useful applications of natural forces led him to become a great inventor and engineer. Indeed, in the field of applied electricity he was one of the greatest inventors who have been known to the world. Electric machinery, electric lighting, electric power, electric welding, electric measurements all received the benefit of his refining experiments and improving inventions. He possessed a truly great creative mind, which is illustrated in the wealth of letters and papers that are in the possession of this Society. His contributions to the publications of the Society were four in number, of which two were after the Centennial date. Of the four, three were of general rather than technical interest. Thomson (like Franklin) was a lover of the meetings of the American Philosophical Society and was an attendant of great constancy, but his influence on the public through the Society may, like Franklin's, be thought of as indirect although large. The Thomson biography now in course of preparation from the background of the Society's treasure of Thomson papers will surely, as in the case of Franklin and other men, strengthen the influence conferred by the Society on the engineering-interested public.

3

We may now turn appropriately to the characteristics of the topics which in that first century of the life of the Society were treated in articles contributed to the Society's publications. In 1768, the date of the merger which produced the American Philosophical Society, every citizen (from cloistered philosophers to rugged merchants and mechanics) recognized the lack of transportation routes in the Colonies, and the American Philosophical Society promptly planned for a committee study of cross-country means for transportation from the Susquehanna River to the Delaware River. Money was raised to enable the committee to direct surveys.

In those days there was important canoe and bateau traffic on the Susequehanna River. Indeed, the Susquehanna was a principal transit artery from eastern Pennsylvania into the back

areas which were becoming important as producing areas. Considering the variable flow of the river and its great stretches where the channel is shallow, this idea of the Susquehanna as an important transportation artery may seem incredible to us, but we live with the trappings of the railroad and the hard-surfaced highway. We disdain transport by canoe and bateau except in the wilderness. It was not so in 1768, which was before the time of either the steam locomotive or the internal combustion engine. The saddle horse and oxcart on rough trails were the competitors of hand-poled or animal-towed boats on the natural streams, and internal water transportation was looked to as of great interest to the Colonies. The committee for the survey remarks that "the river Susquehanna is the natural channel through which the produce of three-fourths of this province [Pennsylvania] must in time be conveyed to market for exportation, and through which great part of the back inhabitants will be supplied with foreign commodities." Here, then, was something to arouse the enthusiasms of Society members. Indeed, it seems to have stirred enthusiasm comparable with that stirred by the prospect of observing a transit of Venus which was about to occur, and for which committees were appointed.

The transportation committee surveyed several canal routes, discussed their relative merits and the relative merits of a sea-level or a lock canal, the estimated costs of the several projects going from over 8,000 pounds to 60,000 pounds. The committee also surveyed routes for wagon roads, one from Peach Bottom on the Susquehanna River in Pennsylvania to Philadelphia, and another from the same origin to Christiana, Delaware, which lies on a tidal stream flowing into the Delaware River. The committee expressed its opinion that the latter road ought to be promptly built.

The committee's report, along with details of surveys and estimates, were lodged with the Society and an abstract was published in Vol. 1 of the *Transactions* (pages 293–300), along with a map of the area. It is interesting to note that Federal Highway No. 1, which now crosses the Susquehanna River at Conowingo Dam, half a dozen miles down river from Peach Bottom, for the greater proportion of its route westward from Philadelphia follows a line that is similar to the one laid out by the committee for the road to Philadelphia; and that the present Chesapeake and Delaware Canal (begun in 1824) follows a route about fourteen miles long from the Delaware to the Chesapeake which is somewhat similar to one of the committee's canal routes. George Washington, a member of the Society, also was a strong proponent of improvement in the Colonies of rivers for transportation and the construction of canal detours around river rapids (which we now would call river canalization), but his projects did not come to realization as waterways because the railroad ultimately came to fill the need more advantageously.

I have outlined this transportation project at comparative length because it shows so vividly the great difference of transportation views in 1769 compared with our own, which are established on the inventions of the steam engine and the internal combustion engine. There can be no doubt that the Society's open discussion of the economic importance of improved internal transportation for the Colony was of great value to the people of Pennsylvania.

Two of the most interesting of the very early contributions to the publications of the Society were by Franklin. They both were printed in Volume 2 of the *Transactions* (old series). The first was a letter to a European (physician to the Emperor at Vienna) dated "at sea," August 28, 1785, on the topic of chimneys—"The Causes and Cure of Smokey Chimneys"—with particular reference to chimneys for residences. This discussion lays down the conditions determining the draft of a chimney in true Franklinian clearness and completeness, and the publication which gave it general circulation had much influence on builders and on occupants of dwelling houses, as I know from the memories of my youth, although I was born eighty years later. In some comments at the end of his letter, on the impact upon poor people of good chimney construction, Franklin remarks, "Those public works, therefore, such as roads, canals, &c. by which fuel may be brought cheap into such countries [*i.e.*, countries not naturally provided with fuel] from distant places are of great utility, and those who promote them may be reckoned among the benefactors of mankind."

In the same volume of *Transactions* is printed another Franklin contribution entitled "Description of a New Stove for Burning Pitcoal, and Consuming all its Smoke" (read in January, 1786). Here is the description of the famous down-draft stove designed to consume coal without discharging smoke. The recognition thus by Franklin

of the principles of combustion applied by carrying the air for combustion through the fire from above, instead of from below, received much attention. The "down-draft grate" at one time became a feature of the furnace of many a steam boiler of size and continued in use where smoke was particularly objectionable until expanding steam uses led to the development of such devices as the underfeed stoker for coal furnaces and finally to the use of automatically-controlled sprayed oil or pulverized coal blown into the fire box.

Thomas Jefferson expounded the requirements for the mould boards for plows in 1799 through a letter published in the *Transactions* of the Society. This may not appear to all to be an engineering device, but when I remember the large industry of manufacturing plows established by James Oliver and others fifty years later, I must confess that Mr. Jefferson made an interesting excursion into the zone of engineering, just as he already had done so ably into the field of architecture.

We need to remember that mechanical power still lagged in 1876, the "Centennial" year, and man's muscles still bore a major part of industrial weariness. The evening sessions of the Society in its early days were lighted by tallow candles.

It is the misfortune of man that, along with our mechanical progression, we have not equally gained in mental acumen or gained strongly in human sympathy. Toward the end of his mould-board paper of 1798 in the *Transactions* of this Society (which was, in fact, prepared as a letter to an English agriculturist), Thomas Jefferson says, referring to the Napoleonic Wars of Europe, "I am fixed in awe at the mighty conflict to which two great nations are advancing, and recoil in horror at the ferociousness of man. Will nations never devise a more rational umpire of differences than force? Are there no means of coercing injustice more gratifying to our nature than a waste of the blood of thousands, and of the labor of millions of our fellow-creatures? . . . . Had the money which has been spent in the present war been employed in making roads and conducting canals of navigation and irrigation through the country, not a hovel in the remotest corner of the Highlands of Scotland, or mountains of Auvergne, would have been without a boat at its door, a rill of water in every field, and a road to its market town. Had the money we have lost by the lawless depredations of all the belligerent powers been employed in the same way, what communications would have been opened of roads and waters!"

Thus Jefferson in 1798! Our today's paraphrase of the sentiment may be in nearly the same words, notwithstanding the increased population and the tremendously increased total wealth now in the world. Indeed, the increase in population and total wealth should make us more emphatic. The anthropologists tell us that fundamental changes of the human mind are deadly slow. The present state of the world makes demonstration. A reader of Tacitus cannot but be impressed that the political corruption and wickedness of the government and the dominant people of Rome near the opening of the Christian era are paralleled in shamelessness by the Hitlerism cult and its leaders today. Our usual rather superficial picture of civilization is too much founded on descriptions of cultures which were only partial civilizations because they failed to recognize a general mutuality of interest among men.

Full civilization will come to us only when we have well developed material and intellectual well-being among large groups of people. To get this requires the contributions of engineering, as Jefferson infers. The establishment and maintenance of community life depend on engineering. Ethical and moral needs arise out of community relations to make civilization. Thus, true civilization is dependent on the fruits of engineering, but it cannot be brought to full embodiment any faster than the manifest intelligence of the people leads the procession toward it.

Franklin's genius had parallelisms with that of Faraday, but Faraday preferred to leave to others the arrangement of economic applications of his discoveries while Franklin craved the pleasure of promptly seeing applications in effect. This same contrast lies between the characteristics of numerous great men of science. The potential utility of the knowledge which he expounded apparently satisfied the intellectual Faraday, but a part in the direct application was needed to fully satisfy the intellectual Franklin. The development of engineering and civilization shows that we cannot increase comfort and security for populations without the efforts of both types of men. But who is to prevent false leaders from robbing populations of the peaceful fruits of such men's efforts!

4

Between 1768, the date of formation of the American Philosophical Society, and 1876, the date

of the Centennial Exposition, Watt's invention of the modern steam engine had come into use; the steam locomotive had been developed; the steamship had been developed; water wheels had been greatly improved; anthracite coal had come into use as a fuel where cleanliness in fuel use was important in this country; oil had been discovered in this country and, in a refined state, had come into use as an illuminant. Discoveries within the domains of physics, chemistry and biology, aided by developments in the field of mathematics, had stimulated activity in invention by scientific men and by active-minded men without formal scientific training. Industries grew up, but nevertheless there was a notable uniformity in the qualities of the engineering activities during that period.

The Centennial Exposition served as an intellectual irritant and aroused strong interest of the American people in industrial accomplishments, which interest extended to scientific circles.

From that point the industrial situation assumed a different guise and engineering took on a new life of initiative and originality. I am of the belief that a good deal of the development of scientific thought relating to the domain of useful applications of science was stirred in this country through the early influence of the American Philosophical Society and other societies of collateral character; but technical writings of engineers later came to be published almost solely in organs of primarily engineering interest. The latter tendency is greatly emphasized now compared with the conditions existing before the days of the "Centennial," and technical matters in engineering are likely no longer to touch the garment of the American Philosophical Society, except lightly.

There is, however, a large immediate field for the most creative minds in the Society to help the world in its great need, by cultivating in coöperation the overlapping areas where engineering, society (civilization), and economics come together. This is a challenge for this Society to maintain a continuing influence in the area staked out by Benjamin Franklin, Thomas Jefferson, Benjamin Latrobe, Moncure Robinson, Elihu Thomson and their creative-minded colleagues. As a field for joint research the area would prove very fruitful. In this, the joint and mutual attention of engineers, historians, economists, social scientists and psychologists is needed for the welfare of the world.

SURVEY OF SCIENTIFIC AGRICULTURE

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(*Read February 13, 1942, in Symposium on the Early History of Science and Learning in America*)

DURING the last one hundred and fifty years science has transformed agriculture from an occupation governed largely by custom and tradition to one in which many of the forces of nature are controlled and made to respond to the wishes of man.

Scientific agriculture, or more properly speaking, agricultural technology, using as it does the biological and physical sciences (biochemistry, genetics, plant and animal physiology, botany, zoölogy, and chemistry), is on a par with the engineering sciences in the use they make of the laws of physics and chemistry through which energy, material, and physical forces are made to contribute to man's well-being.

The agricultural revolution as it pertains to the farm and to the feeding and clothing of humanity is on a par with and by no means lags behind the industrial revolution as it functions through the factory and the transportation system in supplying man with many of his other needs. It heralds the progressive replacement by science of folklore, custom, and tradition as a basis for farm practice.

The period of folklore and farming by tradition covers many centuries. We learn about the earlier agricultural practices from the writings and literature of the times. There is a large bibliography of works on agriculture prior to the middle of the eighteenth century. Part of these works are really poetic, classical essays extolling the virtues of the countryman and the rural way of life. They begin with Hesiod's *Works and Days* and with the Biblical writers.

On the other hand there was another type of agricultural writing which was essentially Aristotelian, and in England there were a number of books of a very practical nature dealing first with the management of the manor and later with general farming practices. The first of these was Walter of Henley's *Husbandry* (15)[1] which dates back to the thirteenth century. Another was Fitzherbert's *Book of Husbandry* (10) which appeared about 1534. Writings of this kind continued until long after the time of Oliver Cromwell. All are part of the record which furnishes an interesting introduction to the later coming of scientific agriculture.

THE AGRICULTURAL REVOLUTION IN ENGLAND

Emergence of the agricultural revolution in England became noticeable early in the eighteenth century with the publication by Jethro Tull of his *Horse-Hoeing Husbandry* (17). In it he reported successful experiments with cultivating the soil between plants and thereby increasing crop yields. The application of science to farm practice expanded subsequently when Robert Bakewell introduced new methods of livestock breeding (11). At about this time there was also a gradual acceptance of Charles Townshend's Norfolk system of alternating the use of land with successive rotations of wheat, turnips, barley, clover, and grass, followed by grazing cattle. We find in the eighteenth-century writings in England frequent references to the experiments and experiences of such men as Tull, Townshend, Arthur Young, Bakewell, and Sir John Sinclair.

MILEPOSTS OF AGRICULTURAL PROGRESS

Historians do not like to divide history into arbitrary periods. Social change is taking place constantly and is complex rather than simple. For this reason I prefer to introduce my topic by setting a few mileposts in the course of the development of agricultural science in this country, realizing that dates are mere mileposts in the flow of change and not arbitrary dividing lines.

The first milepost in the history of scientific agriculture in this country was the organization of the American Philosophical Society in 1743. All students of the development of agricultural science in this country look to this Society with deep reverence. The earliest beginning—the embryo—of the kind of thinking that later developed into sci-

[1] Figures in parentheses refer to literature cited on page 62.

entific agriculture is found in this Society during its earliest years.

The agricultural literature of the American Colonies during the middle of the eighteenth century parallels, in some respects, the output of agricultural writers in England. Two important series of essays were published during this early period —one by Jared Eliot, *Essays upon Field Husbandry in New England* (8), and an anonymous two-volume work entitled *American Husbandry*. Jared Eliot came from a prominent family in Connecticut. He was graduated from Yale College and for a time was a Congregational minister in Connecticut. Later he practiced medicine. He evidently had much interest in agriculture and he observed farmers and their farming. He felt that farmers might benefit from reading about the observations of others in the essays.

Forerunner of Agricultural Societies

The early papers read before the American Philosophical Society leave no doubt that the Society was the earliest organized body in this country in which the scientific view in relation to agriculture germinated. From this standpoint, its most significant period extends from the time of its earliest records until about the close of the War of Independence when agricultural societies began springing up throughout the country. During this period the American Philosophical Society was the only society in this country giving note and attention to agricultural matters. It did so from the time of its organization until its foster child, the Philadelphia Society for the Promotion of Agriculture, was founded in 1785.

Earliest Agricultural Societies

The second milepost commences with the founding and early growth of agricultural societies. So far as the Colonies are concerned, one of the first societies to deal with what I would call scientific agriculture was the society just mentioned, the Philadelphia Society for the Promotion of Agriculture, formed in 1785. Its object was "to promote a greater increase of the products of the land within the American States." For this purpose the society printed agricultural essays and memoirs and offered prizes. A similar organization, the New Jersey Society for the Promotion of Agriculture, Commerce, and Art, had been started in New Jersey in 1781. In the same year that the Philadelphia Society was formed, 1785, the South Carolina Society for Promoting and Improving Agriculture was organized in Charleston. Of considerable importance also was the Society for the Promotion of Agricultural Arts and Manufacture in New York, organized in Albany in 1791.

County Agricultural Societies

The third milepost should be placed somewhere between 1800 and 1810. These years mark the transition from the early beginnings of scientific agriculture in this country, as encouraged by the American Philosophical Society and subsequently by groups like the Philadelphia Society for the Promotion of Agriculture, into a period in which many county agricultural societies were organized in the United States. Many State agricultural societies also came into being.

The county societies usually held meetings in the wintertime. Papers dealing with three aspects of farming and country life were presented. There were papers written by practical farmers telling how they managed farms and produced specific kinds of crops and livestock. There were papers of a semiscientific nature frequently written by doctors, by botanists, or by professors, on what was then called natural philosophy. There also were papers written largely by ministers and teachers. These were religious and ethical in character and dealt with the farm home, flowers and beautification, the upbringing of children, and the various noneconomic aspects of rural life. Usually a fair or livestock show was held during the fall. At these improved livestock was shown, farm equipment was demonstrated, and the products of the fields and the orchards were placed on exhibition.

Along in the 1840's the proceedings of these agricultural societies began to include discussions looking toward the establishment of agricultural colleges. It was frequently pointed out that there were colleges for doctors, colleges for lawyers, colleges for professional people, and by the same token there should be colleges for farmers; and that agriculture should participate in the growing body of science. Thomas Jefferson, the third president of the American Philosophical Society, was frequently quoted in the proceedings, particularly with reference to the necessity for the development of education and science.

The Land-Grant College Act

The next milepost was the signing by Abraham Lincoln, on July 2, 1862, of the first Morrill Act,

usually referred to as the Land-Grant College Act because it provided the enabling legislation under which it became possible for each State to establish agricultural colleges. Earlier in the same year, on May 15, Lincoln had approved the bill creating what was later to become the Federal Department of Agriculture (16).

Agricultural Experiment Stations

In the infant agricultural colleges there was, at first, no body of scientific agriculture. The first professors of agriculture were frequently people who came from the sciences of chemistry, botany, zoölogy, and medicine. Application of these sciences developed gradually. In some of the States agricultural experiment stations were established, and then another milepost was raised when in 1887 an act was passed setting up a Federal Office of Experiment Stations to carry on research in conjunction with the experiment stations and research centers organized by the land-grant colleges in each of the States.

By 1900 the impact of science on agriculture was indeed becoming great. New practices and new modes of production were replacing the older traditional methods. We may, therefore, place another milepost at about 1900 to mark the pronounced transition to the application of science in American farming.

Agricultural Extension Service

Passage of the Smith-Lever Act in 1914 constituted another milepost. This Act provided for the Federal-State Coöperative Extension Service whereby information on agriculture and home economics is, today, made available to farmers in every important rural county. Under the agricultural extension system, farmers and farm families, unable to attend agricultural colleges to learn sound and new practices in formal courses, have had this knowledge made available to them in their own county by agricultural extension agents and, in many counties, also by home demonstration extension agents. These professionally trained extension workers provide farmers with information and assistance on all possible ways in which the application of science to the agriculture of the county can be most useful and promote the general improvement of country life.

Today most of our varieties of field crops and of horticultural plants are the direct product of the science of genetics. Year by year the science of genetics is making over the domestic plants and animals to make them not only more productive and more disease-resistant, but also to improve their nutritional qualities for human beings.

If I were to point out a recent milepost, it would be somewhere within the last decade to mark the growing interest in soil conservation and in the adjustment of our farming practices whereby basic assets of soil may be passed over from one generation to another with a minimum amount of exploitation.

The Oldest Learned Society in America

Having thus briefly sketched the principal mileposts in the development of agricultural science in this country, I shall now confine my discussion to the significance of the early *Proceedings* and *Transactions* of the American Philosophical Society. These papers point to the Society as the first organized body to consider the practical usefulness of science to agriculture. We may make this deduction from the fact that agricultural items were inserted frequently in the published minutes. The published minutes of the years 1768 to 1809 refer to items of agricultural interest more than one hundred and twenty times.

Significance of the Proceedings and Transactions

Most of the minutes published in the *Proceedings* are exceedingly fragmentary but when considered in terms of their context they give the student of the history of agricultural science valuable materials for historical reconstruction.

The earliest minutes of significance to agriculture were reported at the meeting held at the State House, April 19, 1768 (3, p. 13):

> Dr. Williamson acquainted the Society that he had received from Mr. Joshua Gee of Gainsborough in England the Description of a Plough which he believed would be of great use in this Country & that he had a model of the Plough; which was referred to the consideration of the Committee on Mechanics and Husbandry.

On May 18, 1768, a meeting was held in the State House attended by David Rittenhouse and 14 others. The minutes state (3, p. 14):

> Committee on Husbandry recommended to meet at the College, Tuesday May 31, to consider "whether new methods can be fallen upon for preventing the damage done to wheat by the Hessian fly. N.B. M. Du Hamel has written on the subject."

On June 21, 1768 (3, p. 15) the "Committee for Husbandry reported a paper on the Hessian Fly by Dr. Bond which was read and Dr. Bond was requested 'to prepare it for the Press, that it may be communicated to the Public without Loss of Time.'"

The minutes of the same meeting reveal that "A Specimen of Wine made of American grapes with the receipt for making it was communicated by Mr. R. S. Jones."

The following interesting paragraphs appear in the minutes of the meeting of January 16, 1769 (3, p. 24):

> Sample of Chinese Vetches, six bottles of Soy and six pounds of powdered Sago, presented with a letter from S. Bowen of Georgia.
>
> D. Cirillo (of Naples) letter of thanks for being proposed for membership . . . "intimating that they have in Italy a great many trees, fruits and plants . . . of great use to human life, and which may be easily introduced into America."

In the early 1770's a discussion of the growing of silkworms bore witness to concern over American production of an oriental product which has not abated even today. The minutes between 1770 and 1775 reveal a keen interest in domestic silk production and a desire on the part of the colonists to make this into one of the great industries of the New World. On February 2, 1770, recommendations were made calling on the provincial legislature for appropriations with which to establish filatures in and near Philadelphia.

The minutes of March 5, 1773 (3, p. 78) contain the following:

> The Society taking into consideration the great advantage that might be derived to the Public from Premiums granted to encourage the manuring of worn out Lands, which has been done with abundant Success in several parts of Europe by *Marle* & other composts, appointed Mr. Rhoades, Dr. Alison, Messrs Clifford, Mifflin, Alexander & Biddle a Committee to consider of such Improvements in Agriculture as will be most likely to succeed in this Province & of the Premiums to be granted for that Purpose, of which they are to report at the next Meeting.

The following came from the minutes of April 2, 1773 (3, p. 79), which I assume was the report of the above-mentioned committee:

> "Hints for improving the present State of our Agriculture, with a sketch of such Premiums as may be proper to excite the public attention etc." read by Alison & referred to the C. on H. & A. I. to meet next Friday, 7 P.M.

Those minutes which are recorded prior to the establishment of the Society for the Promotion of Agriculture are of considerable historical importance. They have to do largely with suggestions about new plants and their usefulness to human life, insect damage and plant diseases and their prevention, plows, manuring of the land, and general improvement in the growing of plants.

There seem to be no minutes between 1775 and December 1785 dealing with agriculture. The war may have been partly responsible for this. It is significant, however, that on December 16, 1785, a letter and dissertation on the comparative relation of vegetation and different plants was read before the Society. On February 17, 1786, these papers were ordered to be sent to the American Agricultural Society in Philadelphia, inviting a course of experiments and observations on the growth of plants. This seems important because it marks the first recognition in the minutes of the Agricultural Society of Philadelphia.

The *Transactions,* speaking figuratively as well as realistically, served to break ground for the development of agriculture as an applied science in the United States by contributing to a unification of theory and practice. Thomas Jefferson, president of the Society in 1799, explicitly recognized the value of such service when, in the course of a description of a "mould-board of the least resistence" for a plow, he stated (12, pp. 313–322):

> The combination of a theory which may satisfy the learned, with a practice intelligible to the most unlettered laborer, will be acceptable to the two most useful classes of society.

We do not know what thoughts passed through the minds of the members as they read the Virginian's observation, but it is certain that, of the various groups of readers to be found in the country at that time, those among whom the *Transactions* circulated would have been among the foremost in grasping the significance of his remark. Members, interested in both the principles and practice of cultivation of the soil, included Thomas Pinckney of South Carolina, planter and reclaimer of the marshlands; Benjamin S. Barton, physician and botanist; and Timothy Pickering of Massachusetts, who, in his seventy-fifth year, was to win a plowing contest.

Science Replaces Tradition

Although science in the eighteenth century was still in its infancy, a new faith in the perfectibility

of reason and knowledge had appeared among social and political philosophers. Having thrown off the fetters of medievalism, they were busily engaged in extending the systematization of knowledge. In consequence, just as the American Philosophical Society, the oldest learned society on the North American Continent and among the oldest in the world, had its origin in this period of enlightenment, or, as it has been termed, the age of reason, so, too, the roots of scientific agriculture in this country are traceable to the same period.

Of the six committees provided for in the "Laws and Regulations" of the Society as of 1769, there was that on "Husbandry and American Improvements." Premiums were offered in an effort to encourage progress. For example, in 1796, a premium of sixty dollars was to be awarded for "The best method, verified by experiment, of preventing premature decay of peach trees." Papers on a variety of subjects relating to agriculture are to be found in the *Transactions*. New ideas were welcomed by a membership which maintained that (1, pp. xiv–xv):

> The bulk of mankind follow a beaten track. They seldom turn their thoughts to experiments and scarcely ever adopt a new measure, until they are well assured of success or advantage from it, or are set upon it by those, who have weight and influence with them.

Essays on topics so diverse as the distillation of persimmons and a method of drying up the marshes of the maritime parts of North America had been printed as early as 1799. The subjects which were treated include: Viniculture, control of insect pests, the preservation of foodstuffs, the classification of plants, and meteorology.

Nature of Early Essays

Six volumes of the *Transactions*, referred to as the old series, were published between 1771 and 1809. Due to conditions arising from the War of Independence, the second volume was not published until 1786. In the six volumes we find papers that are either definitely agricultural in nature or that pertain to the rural culture of that time as, for instance, Franklin's writings on chimneys. Many of the botanical papers border so closely on the practical side that they should be included among the agricultural papers.

In the general category of agricultural papers I would place at least forty-nine. These range in length from half-page letters to essays of over eighty printed pages. In attempting to classify them we naturally find that some are so comprehensive that they belong in several categories.

A possible grouping may be suggested as follows:[2]

I. *Papers dealing with the cultivation of plants (7 papers).*

This group includes two papers on the vine, two on peach trees, one on hemp, one on sugar in the East Indies, and a rather comprehensive cataloging of foreign plants suitable for the colonies.

II. *Papers dealing with the conversion, preservation, and use of products from the soil (10 papers).*

Here we find treatises on vine and fig culture, on hemp, on drying parsnips, on preparing and boxing seeds and plants for shipment, on extracting oil from plant seeds, on obtaining maple sugar, on the process of claying sugar, on making currant wine, and on the distillation of persimmons.

III. *Papers on entomology in relation to agriculture (7 papers).*

Under this heading we find one paper on the fly weevil (now identified as the Angoumois grain moth), one on preventing peas from being worm-eaten, one on the insect fly-carrier, one on the poisonous and injurious honeys of North America, one on the honeybee, and two on the silkworm and silk industry.

IV. *Papers on plants for medicinal use or primarily of botanical interest (10 papers).*

These include writings that are more or less technical and of wider interest than their application to agriculture. Each, however, sheds some light on the agriculture of that time.

V. *Writings dealing with soil improvement or manuring of the land.*

In addition to several of the papers already mentioned in the first group, there is a paper dealing with the beneficial effects of *Cassia Chamaecrista* in recruiting worn-out land.

VI. *Writings on mechanical devices and equipment (10 papers).*

These include such discussions as Jefferson's description of a mouldboard for a plow, three papers on chimneys and fireplaces, four on gristmills and sawmills, and one on lightning rods.

[2] For titles of papers referred to, see addendum, pages 60–62.

VII. *Writings dealing with animal diseases or anatomy (3 papers).*

There are two rather interesting papers giving the views of that day on infections in the eyes of the horse; also one dealing with observations on two hearts found in one partridge.

VIII. *One paper on weeds.*

This article discusses the eradication of wild garlic.

The Vine and the Soil

In the series of publications, of which only a few can be mentioned, the longest and perhaps most interesting one from the standpoint of practical agriculture is Edward Antill's "Essay on the Cultivation of the Vine" (4, pp. 117–197). This evidently was written by an old gentleman who lived in a suburb of Philadelphia. It is written in the essay style and is interesting reading, particularly to those who are interested in the agricultural essays written more than 100 years ago. From it we get a clear picture of the importance of the vineyard in the farming of that day and the importance of wine in trade and domestic use. Of interest are Antill's observations on the importance of fertilizing the soil and the several methods suggested for "making good store of manure." Antill realized, as did the informed men of his time, that many organic processes take place in the soil and that the decay of organic matter in the soil is an important phase of the eternal cycle of life. But, while he and his associates knew that such organic changes occur, it was not until years later that man discovered effective ways whereby these processes of nature could be facilitated so as to make the soil of greater use. In an essay on the distillation of persimmons (5, pp. 231–234), Isaac Bartram states that the "Persimon Tree" bears a good fruit for distilling liquor and reasons that if every fifty cultivated acres in the province were bordered with "persimon trees we might soon become independent of the West Indies, for the expensive article of rum, and thereby yearly save many Thousand Pounds to this colony."

Perhaps the most advanced stage of scientific knowledge was in the field of plants and botany. A most interesting treatise on this subject is Ellis's "Catalogue of such Foreign Plants as are Worthy of being Encouraged in the American Colonies, for the Purposes of Medicine, Agriculture, and Commerce" (9, pp. 255–266).

The Catalogue is indicative of the great interest people had in plants of great use to human life. Perhaps this interest in nature was deeper because people lived closer to nature and had fewer of the more material interests than we of this generation. Those early searchers for herbs and plants that would benefit human life, however, have their twentieth century counterpart in the search for vitamins in foods and the studies regarding their importance in human and animal nutrition.

Jefferson—Scientist, Farmer, and Patriot

It is probable that Jefferson's "Description of a Mould-board of the Least Resistence" (12, pp. 313–322) is of the greatest historical significance. Jefferson, the third president of the American Philosophical Society, had long been interested in farming. Following his admittance to the bar, he had managed the family estate at Shadwell where his success as a practical farmer is demonstrated by figures which indicate a clearance of some $2,000 annually. The "garden books" which he kept during these years, depict his activities, not only as an agricultural engineer busily engaged in fencing, roadbuilding, and bridging, but as a horticulturalist experimenting with many varieties of vegetables and plants. A neighbor, Philip Mazzei, formerly of Tuscany, was apparently an inspiration in this respect, for, of the dozens of entries of imported varieties of fruits and vegetables in the "garden books" many are in Italian. After the destruction of Shadwell by fire in 1770, Jefferson continued such activities at Monticello until called from his estate to public life.

In the development of scientific agriculture, Jefferson was an internationalist practicing not only the exchange of ideas, but of varieties of plants themselves. Throughout his stay in Europe he noted local ways of husbandry. In England he studied methods of gardening, and on the Continent he took notes on vineyards; the cultivation of olives, figs, and capers; crops such as rice and sainfoin; and industries such as the production of silk.

For friends abroad he obtained American seeds, bulbs, and trees; and, in return, his estate at home benefited by imported species. It should be fruitful to trace the progeny of Jefferson's activity in the international exchange of seeds and cuttings. In the front yard of the old estate, Charlesgift, on the Patuxent River, stands a great old pear tree; near by is a rosebush with a stem as thick as a

man's forearm. The present-day owner estimates the age of the tree, which is a species of pear grown in France, at 150 years. We may speculate that this was a cutting shipped by Jefferson across the Atlantic.

Moldboard of Least Resistance

In recognition of his contributions to agriculture, Jefferson was invited to affiliate with the British Board of Agriculture, and his letter of acceptance, published in the *Transactions* for 1799, to the President of the Board, Sir John Sinclair, was the occasion for the description of a "mould-board of the least resistence." After a description of the functions of the board, the formula for its construction is set forth in minute detail. Many diagrams accompany the detailed explanation. The length of this communication on a subject which had "hardly been deemed worthy of science," Jefferson believed, called for an explanation. He had maintained earlier in the letter (12, pp. 313–322) that agriculture was a subject—

> Of all in the world, the most interesting to man: For none but the husbandman makes anything for him to eat; and he who can double his food . . . deserves to rank, among his benefactors, next after his Creator.

He now noted concerning his description that—

> If the plough be in truth the most useful of the instruments known to man, its perfection cannot be an idle speculation.

Concerning the worth of Jefferson's invention as a contribution to agriculture, one has only to note the difference between plowing in colonial days and that in the nineteenth century to see the immense improvement brought about by application of scientific principles. The clumsy plows of the colonial period, made of wood sometimes protected by iron strips, were unique specimens, for local blacksmiths followed their own ideas of most effective construction. Turning up edged furrows, the primitive instruments required not only the efforts of several horses or oxen, but also the services of two or three men to guide and keep the point in the ground. In contrast, the plows embodying principles of Jefferson's formula were the ones turned out in quantity following Jethro Wood's development of a plow with cast-iron replaceable parts. They sliced the furrows evenly, turning over the sod flat and broken. The friction was so reduced that only half the pull was required. Guidance was so facilitated that one man instead of two or three could do the plowing, and consequently, economy of effort in this fundamental process of agriculture played no small part in making possible increased food supply in the nineteenth century.

Uniting a Valid Theory and Efficient Practice

The description of a "mould-board of least resistence" has been discussed more fully than any of the other agricultural topics included in the *Transactions*, a treatment which is justified on the ground that Jefferson's presentation may be maintained to represent the first application in the United States of a rigid quantitative method of the application of science in relation to agriculture, a fundamental process in the uniting of valid theory and efficient practice wherein lies a primary contribution of the learned Society to the field of husbandry. As theories have been clarified through receptivity and discussion, as thought has been communicated from level to level, and as ideas have been disseminated through publication, so it has been possible for scientific knowledge to be increasingly utilized by the common man. In forwarding this process, other societies, notably the agricultural societies which appeared at the close of the eighteenth century, took their cue from your Society, and this sustained interest in theory as well as practice contributed toward the inclusion of agriculture as a component element in the curricula of higher institutions of learning.

Summary

I have not reported on the papers published after 1809 because by that time, as has been pointed out, the milepost of the establishment of many local societies had been passed, and these societies were flourishing in many States and counties throughout the land. Therefore, the later papers and references, although of considerable scientific importance to agriculture, do not have the significance of those published during the time that the Society stood alone; or, in subsequent years, when the Society was diffusing its influence on the strictly agricultural organizations.

What are some of the generalizations we may make about both the papers which are published in the Society's *Transactions* and the minutes of its meetings up until, say, 1800? I would give them as follows:

1. When put together and viewed in the light of the science of the times, the papers and minutes reveal a good deal of sound, scientific thinking which reflected itself in practical application to agriculture.

2. We find frequent references to good examples of observation of scientific method. Many of the observations were made by men who were primarily interested in science rather than in farming as an economic pursuit.

3. There is a decided absence of papers dealing with animal husbandry and farm management. Practically no reference is made to livestock or to the operation of farms. We may infer, therefore, that the members of the Society were primarily interested in scientific matters rather than in agriculture as a business.

4. The writings reveal a curiosity and an interest in foreign plants and in foreign types of agriculture. There are frequent references to new varieties, to new seeds, new crops, silk, and so forth.

5. The outstanding paper and most significant of all the writings from the standpoint of long-time agricultural progress is Jefferson's paper on the plow. This is perhaps the first important scientific paper of its type produced in the United States.

The emphasis of the members of the Society on scientific method, as evidenced by its *Transactions*, may be said to have been of inestimable importance in breaking ground for the appearance of agriculture as an applied science. I believe that, as copies of the *Transactions* are now comparatively rare and to be found in relatively few libraries, it would be a present contribution of great value to students of the continuity of the science of agriculture in the United States, if these papers or selections from them could be published in a single volume and made available to the public.

Applied Science Extends Beyond the Realm of Things Material

Yet, in stressing the scientific and material contributions of the Society we must not slight the highly important concepts which its members held with regard to faith, morals, traditions, and things of the spirit. Science and technology are part of the picture, but ethics and human values are also part of the picture. As scientific knowledge has accumulated, the need has increased for using the social as well as the physical sciences in strengthening the union between nature and man. These early men of the Philosophical Society had an idea of the unity of man and nature and were interested in a blending of ways and means.

"All the works of nature are linked the one to the other," observes D'Aboville (7, pp. 330–334), "and form a whole in the immensity of which we perceive some points which appear to us detached, because those which unite them are concealed from us.

"The result of this connection is, that no work of nature ought to be neglected, that there is not one but which may derive some direct or indirect utility to man. . . .

". . . Not to lodge every new discovery in this common store, is to squander away riches which we hold only in trust, and in which the most distant generations have an interest. This is criminally depriving humanity of a blessing which is intended for the good of society. . . ."

Sentiments such as these, embodied in the documents of this Society and in companion writings appearing in England and on the Continent at about the same time, reveal a grasp of the concept of the unity of nature, which, like a bridge, unites the material and the spiritual, and provides an opportunity for man to advance nobly in civilization through applying his heritage of ethical and rural virtues to the problems and tasks of a more complicated existence.

There are democratic and spiritual values in the rural way of life. From the time of the earliest poets and philosophers, the principal theme song has been about the essential goodness of the people who live on the soil.

> . . . It is from the tillers of the soil that spring the best citizens, the stanchest soldiers; and theirs are the enduring rewards which are most grateful and least envied. Such as devote themselves to that pursuit are least of all men given to evil counsels (6, p. 20).

So wrote Cato in philosophizing about agriculture 2,000 years ago. This ethical concept of the nobility of those who till the soil was nurtured through the centuries. It found a practical realization in the application of science to agricultural pursuits during the intellectual renaissance of the eighteenth century. Today we know that the agriculturist must combine science with skills and the ethical outlook in order to serve the good of humanity.

We can only conjecture the nature of things and the chain of events that brought together, within a relatively short period of years, men of the

caliber of Franklin, Rittenhouse, and Jefferson, the first three presidents of the American Philosophical Society. The stature of the first two is best described in Jefferson's own words, in his letter of January 28, 1797, in which he modestly accepted his election as President by the Society (13, pp. 251):

> Permit me to avail myself of this opportunity of expressing the sincere Grief I feel for the loss of our beloved Rittenhouse. Genius, Science, modesty, purity of morals, simplicity of manners, marked him one of Nature's best samples of the perfection she can cover under the human form. Surely, no Society, till ours, within the same compass of time, ever had to deplore the loss of two such members as Franklin & Rittenhouse. Franklin, our Patriarch, whom Philosophy & Philanthropy announced the first of men, and whose name will be like a star of the first magnitude in the firmament of heaven, when the memory of those who have surrounded & obscured him, will be lost in the Abyss of time.

Jefferson and Agricultural Science

The breadth of character and interest in science embodied in Jefferson, the third President of the Society, was strikingly referred to by Vice President Henry A. Wallace, when he was still Secretary of Agriculture, on the occasion of the seventy-fifth anniversary of establishing the land-grant colleges and the Department of Agriculture. In connection with this occasion Mr. Wallace placed a symbolic wreath made of cereals and plants produced by genetical science at the tomb of Thomas Jefferson at Monticello. At that time the Vice President said: "It was Jefferson more than any man of his time who foresaw the fruitfulness of the application of science to agriculture. It was Jefferson more than any man of his time who saw the need of a democratized education. It was Jefferson who was the very heart and soul of that democracy, that American ideal which all of us have held so dear."

Science and technology point today to the directions for improving the present and future lot of the farmer and of mankind. A stronger union of valid theory and efficient practice, a more perfect combination of the physical and social sciences, should serve "those who labor in the earth, who"—and here I am quoting a rendition of Jefferson's writings (14, Vol. 3, p. 268) inscribed on the wall of the Science and Technology Room of the Library of Congress—"are the chosen people of God, if ever he had a chosen people, in whose breasts he has made the peculiar deposits for substantial and genuine virtue—its focus in which he keeps alive that sacred fire which otherwise might escape from the earth."

Addendum [3]

Early papers pertaining to scientific agriculture which appear in the *Transactions* of the American Philosophical Society for the period 1771–1809. An attempt has been made by the author to group these according to their significance in special phases of agriculture.

I. *Papers dealing with the cultivation of plants.*

(1) Essay on the cultivation of the vine, and the making and preserving of wine, suited to the different climates in North America. Edward Antill, *Trans.* (o.s.), **1**, 117–197 (1771).

(2) Account of a new species of grape vines. John Jones, *Trans.* (o.s.), **1**, 339–340 (1771).

(3) . . . observations on the raising and dressing of hemp. (This follows immediately Antill's discussion on curing figs.) Edward Antill, *Trans.* (o.s.), **1**, 198–204 (1771).

(4) The mode of cultivating a sugar plantation in the East Indies. Botham, *Trans.* (o.s.), **3**, 79–81 (1793).

(5) Catalogue of such foreign plants as are worthy of being encouraged in the American colonies, for the purposes of medicine, agriculture, and commerce. John Ellis, *Trans.* (o.s.), **1**, 255–266 (1771).

(6) Description of a method of cultivating peach trees, with a view to prevent their premature decay. (Premium awarded.) Thomas Coulter, *Trans.* (o.s.), **5**, 327–328 (1802).

(7) Account of a method of preventing the premature decay of peach trees. (Premium awarded.) John Ellis, *Trans.* (o.s.), **5**, 325–326 (1802).

II. *Papers dealing with the conversion, preservation, and use of products grown in the soil.* (In addition to the two essays on the vine referred to under I.)

(1) Method of curing figs; and observations on the raising and dressing of hemp. Edward Antill, *Trans.* (o.s.), **1**, 198–204 (1771).

(2) Letter to Thomas Jefferson giving an account of the sugar maple-tree of the United States, and of the methods of obtaining sugar from it. Benjamin Rush, *Trans.* (o.s.), **3**, 64–79 (1793).

(3) A letter from the Rev. Jeremy Belknap, on the preserving of parsnips by drying. *Trans.* (o.s.), **2**, 199–200 (1786).

(4) Letter to Charles Thomson concerning a fine oil from bene seed. John Morel, *Trans.* (o.s.), **1**, 239–240 (1771).

(5) Essay on the expressing of oil from the sunflower seed. J. Morgan, *Trans.* (o.s.), **1**, 235–239 (1771).

(6) Letter to Dr. Bond giving an account of oil made from the seeds of the sun-flower. Otto, *Trans.* (o.s.), **1**, 234–235 (1771).

[3] Compiled from the list of publications of the Society.

(7) Directions for putting up seeds and plants so as to preserve them in a state of vegetation for being transported to distant countries. John Ellis, *Trans.* (o.s.), **1**, 266–271 (1771).
(8) Process of claying sugar. Jonathan Williams, *Trans.* (o.s.), **6**, 82–87 (1809).
(9) Receipt for making currant wine. Unsigned, *Trans.* (o.s.), **1**, 246–249 (1771).
(10) Memoir on the distillation of persimons. Isaac Bartram, *Trans.* (o.s.), **1**, 231–234 (1771).

III. *Papers on entomology in relation to agriculture.*

(1) Two letters to Dr. John Morgan on the process of the silk-worm from the egg to the cocoon. Hare and Skinner, silk merchants in London, *Trans.* (o.s.), **2**, 347–366 (1786).
(2) Observations on the native silk worms of North America. Moses Bartram, *Trans.* (o.s.), **1**, 224–230 (1771).
(3) Inquiry into the question whether the *Apis mellifica* or true honey-bee is a native of America. Benjamin Smith Barton, *Trans.* (o.s.), **3**, 241–261 (1793).
(4) Observations concerning the fly-weevil, that destroys the wheat. Landon Carter, *Trans.* (o.s.), **1**, 205–217 (1771).
(5) Letter to Charles Thomson concerning a method of preserving peas from being worm-eaten. Peter Miller, *Trans.* (o.s.), **1**, 243–244 (1771).
(6) Memoir on animal cotton, or the insect fly-carrier. Baudry des Lozieres, *Trans.* (o.s.), **5**, 150–159 (1802).
(7) Some account of the poisonous and injurious honey of North America. Benjamin Smith Barton, *Trans.* (o.s.), **5**, 51–70 (1802).

IV. *Papers on plants for medicinal use or primarily of botanical interest.*

(1) Letters to John Morgan concerning the medical history of the cortex ruber, or red bark. Thomas S. Duché and George Davidson, *Trans.* (o.s.), **2**, 289–293 (1786).
(2) Letter to John Morgan concerning antiseptic virtues of vegetable acid and marine salt combined, in various disorders accompanied with putridity. William Wright, *Trans.* (o.s.), **2**, 284–289 (1786).
(3) Letter concerning a vegetable found under ground. Colonel Bull, *Trans.* (o.s.), **5**, 160–161 (1802).
(4) Botanical description of the *Podophyllum diphyllum* of Linnaeus in a letter to Charles Peter Thunberg. Benjamin Smith Barton, *Trans.* (o.s.), **3**, 334–348 (1793).
(5) Hints relative to the stimulant effects of camphor upon vegetables. Benjamin Smith Barton, *Trans.* (o.s.), **4**, 232–235 (1799).
(6) Memorandum of a new vegetable muscipula. Benjamin Smith Barton, *Trans.* (o.s.), **6**, 79–82 (1809).
(7) First memoir of observations on the plants denominated Cryptogamick. Palisot de Beauvois, *Trans.* (o.s.), **3**, 202–213 (1793).
(8) Index florae Lancastriensis. Henricus Muhlenberg, *Trans.* (o.s.), **3**, 157–184 (1793).
(9) Memoir on the subject of a new plant growing in Pennsylvania particularly in the vicinity of Philadelphia. Palisot de Beauvois, *Trans.* (o.s.), **4**, 173–177 (1799).
(10) Letter to Hugh Williamson containing some remarks on the climate, vegetable productions, etc., of Florida. John Lorimer, *Trans.* (o.s.), **1**, 250–254 (1771).

V. *Writings dealing with soil improvement or manuring of the land.* (In addition to Antill's essay on the vine and several others referred to under I.)

(1) Account of the beneficial effects of the *Cassia Chamaecrista* in recruiting worn-out lands, and in enriching such as are naturally poor: together with a botanical description of the plant. James Greenway, *Trans.* (o.s.), **3**, 226–230 (1793).

VI. *Papers on mechanical devices and equipment.*

(1) Letter to Sir John Sinclair, containing the description of a mould-board of the least resistence, and of the easiest and most certain construction. T. Jefferson, *Trans.* (o.s.), **4**, 313–322 (1799).
(2) Letter to Dr. Ingenhausz, physician to the Emperor, at Vienna, on the causes and cure of smokey chimneys. Benjamin Franklin, *Trans.* (o.s.), **2**, 1–36 (1786).
(3) Calculations relating to grist and saw mills, etc. John Nancarrow, *Trans.* (o.s.), **4**, 348–361 (1799).
(4) Description of some improvements in the common fire-place, accompanied with models offered to the consideration of the American Philosophical Society. C. W. Peale and Raphaelle Peale, *Trans.* (o.s.), **5**, 320–324 (1802).
(5) Letter concerning chimneys, addressed to His Excellency Benjamin Franklin. Thomas Ruston, *Trans.* (o.s.), **2**, 231–235 (1786).
(6) An improvement on metallic conductors or lightning-rods. ("Philo Franklin") Robert Patterson, *Trans.* (o.s.), **3**, 321–324 (1793).
(7) Observations on the theory of water mills, etc. W. Waring, *Trans.* (o.s.), **3**, 144–149 (1793).
(8) Investigation of the power of Dr. Barker's mill, as improved by James Rumsey, with a description of the mill. W. Waring, *Trans.* (o.s.), **3**, 185–193 (1793).
(9) Observations on the theory of water-mills. W. Waring, *Trans.* (o.s.), **3**, 319–321 (1793).
(10) On the mode most easily and effectually practicable of drying up the marshes of the maritime parts of North America. Thomas Wright, *Trans.* (o.s.), **4**, 243–246 (1799).

VII. *Writings dealing with animal diseases or anatomy.*

(1) Account of two hearts found in one partridge. D'Aboville, *Trans.* (o.s.), **2**, 330–334 (1786).
(2) Account of a worm in a horse's eye. F. Hopkinson, *Trans.* (o.s.), **2**, 183–185 (1786).

(3) Of a living snake in a living horse's eye, and of other unusual productions of animals. John Morgan, *Trans.* (o.s.), **2**, 383–391 (1786).

VIII. *Paper dealing with weeds.*

(1) The method of destroying wild garlic. Henry Hollingsworth, *Trans.* (o.s.), **1**, 241–243 (1771).

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SOME EARLY BOTANISTS OF THE AMERICAN PHILOSOPHICAL SOCIETY[1]

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(*Read by John M. Fogg, Jr., February 13, 1942, in Symposium on the Early History of Science and Learning in America*)

Stephen Leacock tells us of a college professor going into a bookstore and promptly being shown *"Universal Knowledge from Aristotle to Arthur Balfour*—seventeen cents." After consideration, the professor turned down the bargain, presumably because he thought the book might be incomplete. I must ask you to let me take my cue from Leacock. I find myself expected in half an hour vividly to portray to you and adequately to discuss as living personalities and as profound scientists more than one hundred botanists who, up to 1875, were members of the American Philosophical Society. Such an epitome would obviously be incomplete. The mere reading of their names would fill the allotted time; and what's in a name?

All I can do, in this contingency, is to quote very briefly the judgments of contemporaries regarding a few who did honor to the Society while honored by it; and in a few cases to note incidents which, perhaps, have been unrecorded. In so doing I originally intended to keep away from Philadelphians for these have been adequately discussed in print by our member, the late Professor HARSHBERGER, while Dr. Pennell has very fully considered some of them in the pages of *Bartonia*. This I have found difficult, however, for the fame of the Medical School of the University of Pennsylvania led to this city most of the earlier American botanists, who followed that science as an avocation.

Systematic botanists and zoologists have a sure method of perpetuating the fame of deserving members, and patrons of science are not forgotten. As LINNAEUS (who appears on our rolls as no. 263, SIR CHARLES A LINNÉ, M.D., elected in 1770) so well put the matter two centuries ago (1737), *"Patrons* are to be held sacred by us, who have helped our science from their private resources, men like the Prince *Bourbon, . . . Sherard, Beaumont, Clifford*. . . . We should not seem to have bestowed too valuable a gift, in offering to such men the name of a plant: nevertheless, they win more honor, to wit everlasting remembrance, through our guileless science than if they set up statues or temples or founded castles or cities."[2] In these times, when statues, temples, castles and cities are so rapidly destroyed, some followers of "our guileless science" earnestly wish that possible patrons would take this truth to heart. The showy plant, POINSETTIA, is known and always will be known by a world which, in general, will know nothing of JOEL POINSETT and his distinguished scientific career; and in the bogs of the Californian Sierras the endemic pitcher-plant, DARLINGTONIA, is known to thousands of plant-lovers who never heard of WILLIAM DARLINGTON.

Even Linnaeus, the father of modern botany and zoology, is a mere name to 999 out of every 1000 people, but every one who visits the northern coniferous forests knows (or should know) the most exquisite of all trailing evergreens, LINNAEA. Of this beautiful, circumboreal plant with deliciously fragrant pink bells Linnaeus himself wrote: *"Linnaea* was named by the celebrated Gronovius and is a plant of Lapland, lowly, insignificant, disregarded, flowering but for a brief space—from Linnaeus who resembles it."[3] Gronovius, elsewhere writing of his pupil, Linnaeus, used superlatives, and when Linnaeus dedicated a genus to another he was fully appreciative; for instance, "COLDENIA, named for Master *Cadwallader Colden* of Coldengham in the American province of New York, who accurately and clearly described the fruiting characters and habits of more than 300 plants of New York according to the sexual system, which manuscript I have many times seen with pleasure";[4] a part of the manuscript is now

[1] In this paper the names of members of the American Philosophical Society or generic names derived from them are printed in capitals.

[2] Linnaeus, *Critica Botanica, trans.* Sir Arthur Hort, 58. Ray Society, London, 1938.

[3] After Hort.

[4] "Dicta a D. *Cadwalladero Colden* Coldenghamiae, in provincia Noboracensi Americes, summo Praefecto, qui plantas Noveboracenses plus quam 300 secundum earum characteres fructificationis & totius plantae ad methodum sexualem accuratissime & acutissime descripsit; quae manuscripta in Bibliotheca Celeb. Dom. Praesidis non sine voluptate multoties vidimus."—Linnaeus, *Amoenitates Academicae*, 1: 119 (1749).

preserved at the Gray Herbarium. From Linnaeus's words one could easily gather that the relatively minor botanical work of COLDEN was of more importance than the tremendous foundations of the science laid by the great master.

When, in 1765, JOHN BARTRAM and his son William discovered one of the handsomest of all large shrubs or small trees, occurring locally on the Altamaha in Georgia, WILLIAM BARTRAM chose "to honor it with the name of that patron of sciences, the truly great and distinguished character, Benjamin Franklin"; as has recently been much emphasized, in the year of Franklin's death, 1790, the species, as a native, was exterminated for commercial gain. Fortunately, however, the fame of BENJAMIN FRANKLIN (I mention him because he was born in Boston) does not rest alone on the genus FRANKLINIA; although it is not improbable that when future generations ask, "Who was DE WITT CLINTON?," the easiest answer may be, "He was the man for whom CLINTONIA was named."

WISTERIA of Nuttall was "In memory of CASPAR WISTAR, M.D., late professor of Anatomy in the University of Pennsylvania, and for many years president of the American Philosophical Society; a philanthropist of simple manners, and modest pretensions, but an active promoter of science"[5]—Nuttall in his dedicatory note spelling the name WISTAR, but calling the shrub WISTERIA. The well-known Osage orange was named by Nuttall MACLURA, with the words: "Dedicated to WILLIAM MACLURE, Esq. of the United States, a Philosopher, whose devotion to natural science, and particularly to the geology of North America, has scarcely been exceeded by Ramond or Saussure in Europe."[6]

In describing the nearly world-wide genus MUHLENBERGIA Schreber wrote: "The genus . . . received its name from me . . . after my venerable friend Herr Dr. HEINRICH MUHLENBERG, first evangelical pastor in Lancaster, Pennsylvania, and head of the German University there, and worthy member of many learned societies, who rendered immortal service to the natural history of North America and particularly to the knowledge of the flora of Pennsylvania and the other United States through the discovery of countless new species and in other ways."[7]

Moench, in setting up the genus LAMARKIA, simply said: "In memory of the celebrated *la Mark*, author of the Flora of France."[8] Everyone then knew LAMARCK, and every modern biologist knows his name, but Moench, unhappily, did not know how to spell it!

Ruiz and Pavon, describing CAVANILLESIA, graciously said: "This genus is dedicated to Master ANTONIO JOSEPH CAVANILLES, naturalist of the city of Valencia, Philosopher and distinguished Botanist, who has contributed through his indefatigable devotion and his various expeditions and excellent work to the illustration of Botany in general, and in particular to this class of plants."[9]

When one of the greatest and most scholarly of botanists, George Bentham, described the genus HOSACKIA, brought back from the West by David Douglas, he said: "Dedicated by Mr. Douglas to DAVID HOSACK, M.D., F. R. S., &c. of New York, a gentleman to whom the scientific men of North America owe the same gratitude as those of England did to SIR JOSEPH BANKS."[10]

So run the dedications to members of the Philosophical Society, from its founder, Franklin, to the last botanist to come into the fold in the first three-quarters of the last century. In describing the genus LAPHAMIA Asa Gray wrote: "I dedicate this genus to I[NCREASE] A. LAPHAM, Esq., of Milwaukee, Wisconsin, author of a catalogue of the plants of that State, and a zealous explorer of its botany."[11]

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[5] Nuttall, *Genera of North American Plants*, **2**, 115 (1818).

[6] Nuttall, *ibid.*, 233 (1818).

[7] "Die Gattung, von welcher dieses wegen seiner Schönheit und des besondern abweichenden Baues seiner Befruchtungstheile überaus merkwürdige Gras die ansehnlichere Art ist, erhielt von mir, als ich die *Genera plantarum* des verewigten **Linné** neu heraus gab, ihren Namen nach meinem verehrungswürdigen Freunde, dem Herrn D. **Heinrich Mühlenberg**, erstem evangelischen Prediger zu Lancaster in Pensilvanien, und Vorsteher der dortigen teutschen Universität, auch mehrerer gelehrten Gesellschaften würdigem Mitgliede; welcher sich um die Naturgeschichte von Nordamerika, und besonders um die Kenntniss der Gewächse Pensilvaniens und der übrigen vereinigten Staaten, durch die Entdeckung zahlreicher neuer Arten und auf andere Weise unsterblich verdient gemacht hat, und auch diese und die folgende Art zuerst bemerkte und mir mittheilte."—Schreber, *Beschreibung der Gräser*, **2**: 141 (1810).

[8] "In memoriam Cel. *la Mark* auctoris Florae Gallicae"—Moench, *Methodus*, 201 (1794).

[9] "*Genus nuncupatum* D. Antonio Josepho Cavanilles, *Hispano-Valentino Philosopho et Botanico insigni, qui indefesso labore, variis peregrinationibus et praestantibus operibus Botanicem universam inter alios, hanc autem ipsam Monadelphiae classem prope unus illustrativit.*"—Ruiz & Pavon, *Florae Peruvianae et Chilensis Prodromus*, 97 (1794).

[10] Bentham in Lindley, *Botanical Register*, **15**: t. 1257 (1829).

[11] Gray, *Plantae Wrightianae*, **1**: 99 (1852).

Of the more than one hundred early botanical members of the American Philosophical Society only a few are not thus commemorated. The later members of the Barton, James and Smith families were out of luck; the names BARTONIA, JAMESIA and SMITHIA were long ago preëmpted.[12] It is not desirable to use our time in repeating the dedicatory comments on the remainder but, in brief, the fame of these members of our society is doubly assured through such genera as BARTRAMIA, BERGIA, MARSHALLIA, FOTHERGILLA, JEFFERSONIA, CUTLERIA, BARTONIA, BANKSIA, THUNBERGIA, BURMANNIA, PALLASIA, PALISOTA, FORSTERIA, BLUMENBACHIA, ROXBURGHIA, LEWISIA, HUMBOLDTIA, COLLINSIA, SWARTZIA, MICHAUXIA, CORREA, RETZIA, CLINTONIA, SCHWEINITZIA, STEINHAUERA, BLAINVILLEA, ELLIOTTIA, BRONGNIARTIA, AFZELIA, CONRADIA, ROMANZOFFIA, OERSTEDIA, HORSFIELDIA, SHORTIA, AGARDHIA, and so on through the great botanists for forty years more.

I now come to a few botanical members of the Society in the past, whose work I have come specially to know because my own is so largely built upon their foundations, or because of unrecorded incidents which, if not noted, might soon be forgotten.

THOMAS NUTTALL (1786–1859), a young printer of Yorkshire with a passion for exploration and natural history, came to Philadelphia at the age of twenty-two, in 1808. Immediately fascinated by a new and undisturbed flora he started to master it, introducing himself and his problems to Professor BENJAMIN SMITH BARTON, who shared with him his intimate knowledge of plants. Beginning with Barton, Nuttall made the acquaintance of many prominent scientists, his biographical memoir in the *Proceedings* of the American Philosophical Society being prepared by the patron and friend of his later years in Philadelphia, ELIAS DURAND.[13] From this sympathetic and vivid account of Nuttall I draw some items.[14]

After local botanizing on the lower half of the peninsula between the Delaware and Chesapeake Bay, with a trip to tidewater Virginia and North Carolina, Nuttall joined, in 1809, the exploring party of John Bradbury to the Platte and the Mandan region, only through rare good fortune returning alive. Reaching Philadelphia in 1811, he settled down for some years, to work out his vast collections, and in 1818 he published his remarkable *Genera of North American Plants,* a closely printed two-volume work such as had never before been produced in America in the botanical field. So thorough was Nuttall in the preparation of this work that the incomparably accurate scholar, JOHN TORREY, in 1824, thus dedicated his own great *Flora of the Northern and Middle Sections of the United States:* "To Thomas Nuttall, Esq. Fellow of the Linnaean Society of London, Member of the American Philosophical Society, &c. &c. this work is respectfully inscribed, as a tribute of respect for his scientific attainments, and as a token of esteem for his personal character." Such a tribute from such a high authority is rarely exceeded.

In the autumn of 1818, with financial support secured by CORREA DE SERRA, ZACCHEUS COLLINS, WILLIAM MACLURE and JOHN VAUGHAN, Nuttall started for the Arkansas country, again nearly losing his life, but in the spring of 1820 he safely reached Philadelphia. There followed notable reports upon the Arkansas region, its aboriginal population, flora, fauna, and geology. After the death of Professor WILLIAM DANDRIDGE PECK, who was the first to teach botany at Harvard (except for some lectures by BENJAMIN WATERHOUSE), Nuttall was selected to fill his position. The fund of the professorship of Natural History being very meager (I know, for I hold that chair), Nuttall was let off with occasional lectures in botany and with the development of the Botanic Garden. Tradition has it that he was not a good lecturer, his life having been so much of a hardy pioneering type. He was described as stout and stooping, moving like an Indian, and morbidly unsociable,[15]

[12] If their taxonomic friends had sufficient originality, the dedications to the Smiths and the Browns might still continue. Witness the genus *Nebrownia,* named for the comparatively recent English botanist, N. E. Brown; or this delightful commemoration of the brothers, James Drummond, famous botanical explorer in Australia, and Thomas Drummond, equally distinguished explorer in America, from the Arctic to the Mexican Boundary. There already being a genus *Drummondia,* Harvey dedicated to the two brothers the genus *Drummondita,* saying: "Lest however there should be any confusion, I have adopted the termination *'ita'*—being an I for James, and a T for Thomas!"

[13] Elias Durand, "Memoir of the late Thomas Nuttall," *Proc. Amer. Philos. Soc.,* 7: 297–315—repr. 1–19 (1860).

[14] Pennell's "Travels and Scientific Collections of Thomas Nuttall" in *Bartonia,* No. 18: 1–51 (1936), is very detailed.

[15] Although Nuttall was shy and reputedly unsociable, he did not hesitate, when justified, to speak his mind. One of the most active and apparently unscrupulous early Philadelphian botanists was Frederick Pursh. Charges of his professional dishonesty were frequent, and Nuttall added his own in his discussion of his proposed genus *Bartonia* of the Loasaceae: "In reply to the insinuations

except with the very rare individuals who struck a sympathetic chord.

Of this period Durand wrote: "While at Cambridge, Mr. Nuttall led much the same retired life he had done in Philadelphia; he made few acquaintances, and the late Mr. James Brown was, perhaps, his only intimate friend. The house which he then occupied . . . retains yet traces of some ingenious arrangements to favor his recluse habits." Durand, in the last statement, left much to the imagination. The fine old house, afterward the almost life-long home of Professor and Mrs. Asa Gray, exhibited Nuttall's method of avoiding social intercourse. When I first came to Cambridge in the spring of 1891, Mrs. Gray, the former Jane Loring, known to a generation of college students as "Lady Jane Gray," was full of reminiscences of still earlier days. She had retained Nuttall's trap-door. In the corner of the large living-room and study there hung a stout cord. Whenever Thomas Nuttall heard the garden-gate click or a step on the walk he pulled the rope and the trap-door in the ceiling opened nearly to the floor, with a step-ladder on its upper side. Then Nuttall disappeared and was never at home. What a blessing this simple device would be in some modern offices and studies! Yet, as Mrs. Gray used to say, Nuttall complained that Cambridge people were unsociable.

At Cambridge Nuttall kept up his productive activity, although, because he was not exploring, he complained of merely vegetating. During the vegetating period, however, he produced his famous two-volume, illustrated *Manual of the Ornithology of the United States and Canada,* a textbook on Systematic and Physiological Botany, and numerous scientific reports and memoirs, some published by the Philadelphia Academy of Natural Sciences, and at least one in the *Transactions* of the Philosophical Society.

After 11 years of thus vegetating in Cambridge, the home of the great explorer of the Northwest, Capt. Wyeth, Nuttall was seized with wanderlust. He yearned to join Wyeth's second expedition. Unable to secure leave of absence from Harvard for so long a journey, Nuttall resigned, returning to Philadelphia until the expedition started, with financial aid from the Philosophical Society and the Academy of Natural Sciences. The arduous experiences were so adequately described by Nuttall's companion and friend on the expedition, John K. Townsend, that I need not attempt a summary of them here.

Later expeditions, to Hawaii and California, with a trip around Cape Horn (described in Dana's *Two Years before the Mast*), followed, and finally, coming into the small estate of an uncle in England, Nuttall closed his long career of pioneer exploration of the New World. Really happy only on the trail or when botanizing or studying birds or when working over his collections, mostly unhappy and a recluse when in the habitations of men, he returned to England. As Durand puts it: "Poor Nuttall! Yes, indeed, he may well say 'almost an exile, to the land of his nativity.' He had left it thirty-four years past, and was returning almost a stranger to its people and customs." Taking up his English estate, Nuttall wholly abandoned his old pursuits, turned to gardening; whereas he had during a third of a century covered on foot, by canoe or on horse-

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of Mr. Pursh . . . I must here remark, that he could not possibly have had any authority to assert, or even suppose me capable of disputing with the late indefatigable and unfortunate M. [Capt. Meriwether] Lewis, the discovery of this plant; this charge is merely a subterfuge. Mr. Pursh, before he had perused the notes which I had made from the living plant on the Missouri, with an intention of rendering them public, had not then, by his own acknowledgment, any thing like materials for publishing this genus, my friend A. B. Lambert, Esq., Vice President of the Linnean Society, can also aver the truth of this statement. Mr. P. possessed merely an imperfect capsule of the plant, which M. Lewis had collected while descending the Missouri, he not having seen it then at the time of flowering; the collections made by that gentleman while ascending the Missouri were unfortunately lost, and it is only in that collection, according to the time of the year, which he could possibly have had flowering specimens, of this late autumnal plant. This unfortunate want of fidelity, prevented me from communicating to Mr. F. Pursh, many of the plants which now appear in this work. Appeals to the public are to me extremely irksome, but silence on such an occasion would have been indeed the most degrading condemnation, and a tacit submission to reiterated injustice. It was not surely honourable in Frederick Pursh, whom I still esteem as an able botanist, to snatch from me the little imaginary credit due to enthusiastic researches made at the most imminent risk of personal safety!" After living in Philadelphia for some years, beginning in 1799, Pursh went to England in the winter of 1811–12 to work up his amazingly brilliant 2-volume *Flora Americae Septentrionalis* (1814). He was a hopeless drunkard and tradition in London has it that, taken in hand by two patrons, Sir Joseph Banks and A. B. Lambert, Esq., he was kept under lock and key, with carefully restricted fluid stimulation, and allowed an hour's walk a day, under guard to insure his return to his labors. Needless to say, his standing among Philadelphia botanists did not result in his election to the American Philosophical Society. The Philadelphia Academy now possesses the most complete collection of Pursh's types.

back vast unmapped regions from New England to Florida, west to the Columbia and California, his only expedition in his last seventeen years was a single visit to Ireland. The flame had diminished, and we are fortunate that its vivid burning was during the period between Nuttall's quitting his printing-press and his final return to the garden at Nutgrove.

The hundreds of types or original specimens upon which Nuttall based his genera and species are largely at a few institutions. His personal herbarium, carried back to England, is now at the British Museum of Natural History. The Academy of Natural Sciences of Philadelphia is rich in Nuttall's material. Some went to Torrey in New York, and two other series, not generally known, are in the Gray Herbarium at Cambridge. The first includes hundreds of Nuttall plants with his original tiny labels, many of them obvious types. They were presented to Harvard by Nuttall's great patron and admiring biographer, Elias Durand. Of this series Harshberger wrote, in 1899: "It probably contained a more complete set of Nuttall's collections than any other, not excepting the herbarium of the Academy." The letter-file at the Gray Herbarium contains a protest from an officer of the Academy, asserting that these specimens really belong to the Academy and should be returned there. In view of the fact, however, that, as patron and close friend, Durand had his pick of Nuttall's materials and then definitely presented them to Asa Gray, the presentation slip reading: "COLL. NUTTALL. Presented by Elias Durand, 1866," I can, as Director of the Gray Herbarium, only offer my condolences to the Academy. For many years Elias Durand had been a curator of the Philosophical Society and chairman of the Academy's committee on botany. When he placed this series of Nuttall's plants in Cambridge Durand, obviously, was familiar with the Philadelphia institutions. A second series, with many types, evidently received from Nuttall by his Boston patrons, went to the Boston Society of Natural History and is now deposited at the Gray Herbarium. Whether another series of Nuttall's plants went to the Museum in Paris, to which Durand transferred his private herbarium in 1868, I do not know.

References to Durand inevitably bring to mind the remarkable way in which old and valuable collections are often rescued from pending destruction. In the *Journal* of the Academy of Natural Sciences of Philadelphia, in May, 1856, Durand published his "Plantae Kaneanae Groenlandicae," an enumeration and discussion of the Polar plants brought back by the KANE Expeditions. Seventy-two years later, one of the most fastidious and scrupulously groomed of amateur botanists, the late Nathaniel Thayer Kidder, walking up Chestnut Street, on Beacon Hill in Boston, was unceremoniously tempted by a familiar-appearing package on top of a barrel of ashes, ready for collection by the city ash-trucks. Poking at it with his stick, he verified his suspicion. His conventional propriety further overruled by curiosity, he rang, in the early forenoon, at the door opposite. The two elderly ladies living there explained that the package was discovered in a clearing out of the attic, and that Kidder might have it. The presentation slip in the Gray Herbarium does not mention Kidder. It reads: PLANTS OF GREENLAND collected by BENJAMIN VREELAND, M.D., on the KANE "GRINNELL" EXPEDITION, 1850 (Presented 1927, by Misses Edith and Frances Fisher). The labels with their identifications were written by Durand. This was evidently a duplicate set presented to Dr. Vreeland.[16]

JACOB BIGELOW (1787–1879),[17] son of a country parson in eastern Massachusetts, although best known for his distinguished services in medicine, was the first native of New England to write extensively upon the flora of that area. After graduating from Harvard College he went to the University of Pennsylvania to attend medical lectures and on March 6, 1810, wrote to his parents: "The medical lectures being concluded, our professors have set their mills a-going for manufacturing doctors. Happening to pass by the university today, I got one foot entangled in the mill, and not being able to disengage myself, was drawn in and ground over for about an hour, then came out Dr. Bigelow."

[16] Those who attended the Officers' Dinner at Cambridge University during the International Botanical Congress of 1930 will never forget the graceful and delightful address of the President of the Congress, the late Sir Albert Seward. Referring to the wonderful array of portraits of Trinity College men which hung about us in the dining-hall (Lord Bacon, CHARLES DARWIN and others) Seward pointed to a beautiful painting of Byron. That, he told us, was found in a rubbish-pile, waiting on a dock in London to be dumped into the harbor. Obviously, he said, since Byron was expelled from Trinity for writing sacrilegious and immoral verses, it could not be placed in the chapel; the dining-hall, presented by Henry VIII, seemed the appropriate place.

[17] Derived largely from Memoir of Jacob Bigelow by George E. Ellis. Cambridge, 1880.

Returning to Boston, young Dr. Bigelow struggled for a living; but his keen interest in everything from the classics and poetry to surgery and botany, with mechanical and artistic ability which brought him the laying out of beautiful Mt. Auburn Cemetery, soon won him hosts of friends. He was promptly recognized as a man of unusual capacity in many fields, socially the exact opposite of Nuttall, who soon succeeded him in teaching botany at Harvard. Of his medical career it is impossible for me to speak, but his botanical work I can fully appreciate. In his own words, "My acquaintance with botanical science began with a love of plants conceived during my earliest education in the country, and afterward improved by an attendance on the lectures of Dr. B. S. Barton, in Philadelphia, and of Professor Peck in Cambridge." Although Bigelow thus acknowledged his indebtedness to BENJAMIN SMITH BARTON and to WILLIAM DANDRIDGE PECK, as improving the botanical education begun as a country boy, he later stated, with the humor for which he was famous, that "His first lesson in botany . . . was from 'The most learned inhabitant in Sudbury.'" Little Jacob, carrying to this man a flowering stem of Star of Bethlehem, *Ornithogalum* (of the lily family), asked what it was, promptly receiving the reply, "Why, you little fool, that's grass." From then on Bigelow was a doubter.

In 1812 Bigelow joined Professor Peck in conducting a popular course in botany. So successful and stimulating were his lectures that botany came into great vogue about Boston and, in order to meet the insistent demand, Bigelow prepared his *Florula Bostoniensis* (1814), the third and last edition, extended to cover much of New England, published in 1840. In 1815 Bigelow was appointed Lecturer on Materia Medica and Botany in Harvard University and he soon found it important to draw together the accumulated matter in this double field. This resulted in the first volume of his still highly informative *American Medical Botany* in 1818. He published before the invention of lithography, made his own colored plates and personally developed a method for reproducing them in color—an example of the direct method of meeting difficulties which characterized his whole life and so promptly marked him as a leader in medical education and practice. In too many of our modern descriptive works in botany authors are content to draw whenever possible from the writings of others. Not so Bigelow. His descriptions were original, drawn from actual material before him, and, when we wish quickly to learn the technical characteristics of some New England species, we automatically turn to Bigelow. If he knew the plant his description can be trusted. Although this statement rests upon my own experience, since writing it I have found that in 1879 ASA GRAY similarly said, "the descriptive matter must have been original; and it shows that aptitude for seizing the best points of character or most available distinctions, and of indicating them in few and clear words."

With a sound basis in Latin, medicine, chemistry and botany, Bigelow was the obvious head, and therefore "laboring oar" of the group which in 1820 prepared the American Pharmacopaea, and in systematic botany and phytogeography he was a prophet. In 1816, in a paper known to but few, because hidden in a medical journal, he made a comparison of the native floras of temperate eastern North America and temperate Europe, emphasizing that, whereas the species of the Arctic regions would doubtless prove identical on the two continents, those restricted to temperate regions would be found to differ. This, I believe, is one of the first phytogeographic predictions made by an American, and a century and a quarter since its proposal we are still finding new verifications of the correctness of Bigelow's intuition. The Dutch botanist, Van Steenis, says: "Genius in general is characterized by a high power of intuitive generalization, starting with inadequate facts." Bigelow was surely a genius. Working in a period when it was sacrilege to doubt the immutability of species, he wrote in 1824, in the 2nd edition of *Florula Bostoniensis,* concerning the goldenrods and asters (*Solidago* and *Aster*): "Among the species there are a vast variety of hybrids and subspecies which the labors of botanists have not yet been able to reduce under permanent characters, though names without number have been applied to fugitive varieties. The single species found in Great Britain is acknowledged to be one of the most difficult plants 'to define or understand.' The same remark is applicable to a great part of the American species." No taxonomist or acute field-botanist today would question the truth of Bigelow's observation; at the time he published it, it was heresy. Bigelow's memory among botanists is assured in the large genus BIGELOVIA, golden-flowered *Compositae,* found all the way from southern Canada to the Andes of South America.

JOHN TORREY (1796–1873), of New England ancestry but born in New York, was a chemist by profession, a botanist by preference. His living had to come from the more remunerative field. It is, therefore, remarkable that, loaded by university and governmental work in chemistry, he became for many years the outstanding American botanist. His botanical papers began in 1817 and their size and importance grew through the first volume of his *Flora of the Northern and Middle Sections of the United States* in 1824, then his monumental two-volume *Flora of the State of New York* (1843), along with his joint work with his student, ASA GRAY, the *Flora of North America* (1838–1843), the most searching treatment of North American plants up to that time. Torrey was exact, scholarly, a kindly and devoutly religious man, and in the goodness of his heart ready to help everyone. Naturally, then, his professional obligations, and those constantly thrust upon him by state and federal government, in chemical work, crowded out the botany. What I have said of Jacob Bigelow I repeat of John Torrey. His descriptions are wonderfully vivid and accurate. When I suspect that an eastern North American plant has been wrongly identified with one from Europe I first seek the best description I can find of the latter and then closely compare it with the detailed diagnosis in Torrey's *Flora of the State of New York*. If no discrepancy is found I give up. Asa Gray thus closed his biographical note on Torrey: "Thirty or forty years ago a new and remarkable evergreen tree was discovered in our own Southern States, which it was at once determined should bear Dr. Torrey's name. More recently a congener was found in the noble forests of California. Another species had already been recognized in Japan, and lately a fourth in the mountains of Northern China. All four of them have been introduced, and are greatly prized as ornamental trees in Europe. So that, all round, *Torreya taxifolia, Torreya Californica, Torreya nucifera,* and *Torreya grandis*—as well as his own important contributions to botany, of which they are a memorial—should keep our associate's memory as green as their own perpetual verdure."

The author of the latter tribute to his early teacher, ASA GRAY (1810–1888), was, like Jacob Bigelow, a country boy, who through keen intellect, steadfast purpose, personal charm and perpetual industry rapidly rose in repute and dignity through an assistantship to Torrey, collaboration with him on his most scholarly botanical work, a life-long professorship at Harvard, acknowledged leadership for half a century in American botany and the distinction of being the first (I think the only) American botanist honored in the Hall of Fame in New York. A boy born in a remote rural community in the Mohawk Valley, who at the age of 28 was in Europe hobnobbing with the great, SIR WILLIAM JACKSON HOOKER and his son Joseph (later SIR JOSEPH HOOKER), Francis Boott, Robert Brown (described by HUMBOLDT as "botanicorum facile princeps"), Lambert, Adrien de Jussieu, Decaisne, BRONGNIART, Boissier, ENDLICHER, VON MARTIUS, A. P. and Alphonse DE CANDOLLE, Schlechtendal, Kunth—a boy who on his first European trip became the friend and correspondent of such men was no ordinary person. Repeated visits to the European herbaria extended the friendships and when CHARLES DARWIN advanced his daring and revolutionary ideas his great champion in America was the devout member of the Congregational church, his close friend, Asa Gray. The long and often very bitter controversy over evolution is partly over. Its history can hardly be considered here. One small item in connection with it, however, should be recorded. LOUIS AGASSIZ, also at Harvard, was one of the most bitter opponents of Darwin's heresies, just as the equally earnest Christian, Gray, was Darwin's outstanding supporter. The differences between Agassiz and Gray grew almost to estrangement, for Gray was keen in debate, a master of satire as well as simple logic and marshalled facts. In recording this item which, I think, is not in print, I will be pardoned reference to myself. On his 85th birthday I called upon my old professor of chemistry, the late and beloved Charles Loring Jackson. In the course of the conversation he referred to some of my less placid papers and said: "I am glad to see that you know how to use oil of vitriol." "Oil of vitriol?" I asked. "Why," he replied, "when Uncle Asa (Jackson was a nephew of Mrs. Gray) and Louis Agassiz were at sword's point, they were both on the programme of a national meeting, to discuss evolution. When they got home the family said: "We hope, Uncle Asa, that you poured oil on the troubled waters." "I did," was the reply, "oil of vitriol and plenty of it."

That must be my humble contribution to Grayana. The greatness of the man, his lovable and simple manner, his contempt for fraud and his vast life-work in making known the flora of North America are too much to discuss in five minutes.

They speak for themselves. It is doubtful if they will soon be equalled. Two great services to American botany by Asa Gray and his beautiful wife and constant companion, Jane Loring Gray, are little known. Visiting European herbaria in search of the types of North American plants, Gray began with the first trip voluminous memoranda upon them. At a time when the type-specimens in the Old World collections of foreign plants were there not fully appreciated, he was granted permission to take characteristic fragments. Consequently, his herbarium contained many hundreds, if not thousands, of quite identifiable bits, preserved in paper pockets, of types of Linnaeus, Walter, Michaux and many others—fragments which, if Europe keeps on destroying her most precious collections, may sometime be all that is extant to show for them. The notes, too, are a constant source of verification. While Gray was occupied in the museums, Mrs. Gray was browsing in old bookshops, gathering manuscripts, letters and portraits of botanists from Clusius and Fuchs to the middle nineteenth century. These facts I put on record as further evidence of a far-seeing and scholarly scientist.

Like others of the period CHESTER DEWEY (1784–1868) was chemist, geologist and botanist in one. Unlike most, however, he started as a clergyman, but he soon felt called to be an educator. A native of Berkshire County, Massachusetts, he started his teaching career at Williams College. Then, after some transitional positions, he spent his last years at the University of Rochester, New York. In botany Dewey is known chiefly as a tireless student of our largest genus of flowering plants, the intricate and highly technical genus *Carex*, the sedges. For 43 years (1824–1867) he contributed with almost clock-like regularity to the pages of the *American Journal of Science* a series of papers entitled "Caricography." Recognized by his contemporaries as one of the few outstanding authorities on a perplexing and not at all showy group, he seems, at the same time, to have been looked at askance by the more cautions of them. Now, three-quarters of a century after his death, many more of his specific propositions are recognized than fifty years ago; in other words, Chester Dewey's intuition was surer than his colleagues supposed.

My reason for selecting Dewey as the last botanist I shall specially discuss is, that the history of his important collections, containing 97 types of species and varieties proposed by him, is typical of that of many small collections which, after the death of their assembler, fall into unappreciative hands. A loyal graduate of Williams College to which he had consecrated his early years of teaching, at his death in 1868 Dewey bequeathed his very precious herbarium and remarkable library to the College. The Catalogue of Williams College for 1868–69 (p. 24) contains this statement:

> The Botanical Collection has just been enriched by the extensive Herbarium of Carices, presented to the College by the late Professor Dewey. This herbarium is one of the most complete in the country. A valuable Library on Carices was also presented by Professor Dewey and accompanies the herbarium.

This or similar statements regarding the possession by Williams College of the Dewey *Carices* and library continued in subsequent catalogues, at least to that of 1876–77.

In April, 1900, needing to study certain of Dewey's types, I wrote to Williamstown, to inquire if I might there examine them. The reply from the late Professor Samuel F. Clarke was discouraging:

> April 5, 1900. . . . I have no recollection of having seen the "Dewey collection." . . . Professor Chadbourne also had something of a collection, which was later removed from here, some 12 or 15 years ago. I do not know where it is.

As if further answering my query, I very soon received from the late William C. Strong, professor of physics and other sciences at Bates College in Lewiston, Maine, a letter, stating that in a wooden case in an attic of the Science Building there was an old collection of "grasses" made by a man named Dewey. They were poor specimens and of no interest (to a physicist), so they were going to burn them up. Suddenly remembering my interest in such unpractical things, he was writing to ask if I would like to have some of them!

I hastened to Lewiston. There, in pigeon-holes in the attic, was Dewey's herbarium, the specimens fastened by exceedingly long pins run through the labels, to sheets of coarse gray paper. Since the sheets had originally been too large for the pigeon-holes, the so-called curator had pruned them down with large shears, indiscriminately chopping specimens and labels. They could then be rammed into the pigeon-holes. There the responsibility ended. Now, the pigeon-holes being needed for something worth while, a bonfire of the useless "grasses" was next in order. I dined with the College President and explained to him that, although these speci-

mens were not wanted by his institution, it would be most unfortunate to destroy them; in the hands of a specialist on the sedges they would be of the greatest scientific importance. My diplomacy was too open, for I promptly received the reply: "If these Dewey plants would be of value to Harvard University, they are certainly of value to Bates College." The bonfire was immediately abandoned.

Four years later, at the beginning of a new regime at the College, the question was reopened, the exchange proposed was accepted, and in February, 1904, I returned to Lewiston, to pack the collection for shipment to Cambridge. Pasteboards around some of the packages bore, in the characteristic hand of Sereno Watson (former Curator of the Gray Herbarium) the address, "President Paul Ansel Chadbourne, Williams College." Since Watson's remarkable botanical career began with the CLARENCE KING Exploring Expedition and he had come to Cambridge several years after the death of Chester Dewey, it was evident that I had stumbled upon a first-class mystery.

Briefly, the plot worked out as follows. Watson brought to Cambridge, to work up with the aid of Asa Gray, the vast collections of himself and William H. Brewer secured on the Clarence King Expedition. From this resulted the *Botany of California* (1876–80). William Boott of Medford treated *Carex* in that work. Consequently, since it was desirable to see Dewey's material in the genus, this was borrowed; and after Boott had finished with it, Watson returned it to Williamstown, addressed to Chadbourne, himself an amateur naturalist, whose considerable herbarium had also been deposited at Williams College.

In the *American Naturalist,* **10**: 370, for June, 1876, there appeared the following item:

AN HERBARIUM FOR SALE.—An herbarium containing specimens illustrating six thousand species of plants, is offered for sale. Full particulars can be obtained from President Chadbourne, of Williams College, Williamstown, Mass.

Seeing this, a gentleman in Lewiston bought Chadbourne's herbarium for Bates College, Dewey's herbarium arriving with it.

The sequel is short. Receiving a request for the loan of the type of *Carex barbarae* of Dewey, I went to the portion of his collection where it should be found. There was nothing in the cover. Examination of the organized Gray Herbarium, however, revealed it, properly mounted and bearing the printed presentation-slip, "ex Herb. WILLIAM BOOTT." William Boott had not returned the Californian material of Dewey; but in the end the wanderers and the original collection were reunited. When I reported the incidents to Williams College I received a gratifying letter from Professor Clarke, stating that they were glad that the runaway collection was now safe in a haven where it would be cherished and constantly consulted.

New England is proud of the high scholastic records of Williams College and Bates College. The purely technical and extremely specialized collections of Chester Dewey were, from the start, quite inappropriate to their collegiate work. They should never have been buried at either institution, where the need would have been better filled by a modern and more representative collection. Many such histories of small but technically important collections, inappropriately tucked away unwanted, will be recalled; it is not only crown-jewels and old objects of art which have had devious and shadowy journeys. One of the most important functions of our better equipped research-establishments and such repositories as the American Philosophical Society is to see that they, small and forgotten libraries, and significant but unappreciated manuscripts do not reach the waste-lot, the furnace or the bonfire.

A GENERALLY OVERLOOKED RAFINESQUE PAPER

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ABSTRACT

A consideration of an overlooked paper published by Rafinesque in 1834, containing 31 generic names and 15 binomials that have been entirely overlooked by all botanists. Some consideration is given to the reasons why Rafinesque's work was ignored by his contemporaries and successors. Fourteen Rafinesque titles are added to Fitzpatrick's bibliography of 1911, eight of which were actually published by Rafinesque between 1820 and 1839. Thirty-four additions are made to Fitzpatrick's *Bibliotheca Rafinesquiana*, nine of which appeared between 1819 and 1901, the remainder between 1912 and 1942. Rafinesque's papers that appeared in various American and European periodicals are briefly discussed, as well as his personally sponsored serials. There follows a brief consideration of somewhat over thirty privately sponsored botanical periodicals initiated in the United States between 1875 and 1942. The problem of determining the exact status of the many hundreds of new genera and several thousands of new species of plants that Rafinesque described is considered, and some of the difficulties are pointed out, with suggestions as to how the task may possibly be accomplished. The paper closes with a transcript of Rafinesque's statements regarding the new genera and new species proposed by him in 1834 (*Act. Soc. Linn. Bordeaux*, **6**: 261–269, 1834), and with a list of additions and corrections to *Index Kewensis* based on this study.

IN November, 1834, there appeared a short botanical paper by Rafinesque [1] that has apparently been overlooked by all botanists, including Rafinesque himself if we may judge by the fact that in some of his later works he republished items that appear in this paper but without references to it. The paper is dated at Philadelphia, May 1, 1834, and, like many of Rafinesque's botanical contributions of this period, leaves much to be desired. It falls in the same category as his reviews of the publications of his contemporary American and other botanists,[2] and the publication of these could only antagonize his coworkers. In these reviews, where Rafinesque differed from the various authors in their conceptions as to the limits of genera and species and in the application of scientific names, he published, in the course of his cursory remarks, scores of new generic and specific names. Although these are difficult to detect, most of them have been included in standard indices, except those that appear in the paper on de Candolle, the subject of this article. In this most casual method of publication of new names Rafinesque was the pioneer, but fortunately for the taxonomists and the systematists his innovation was not followed. In our times it is an unwritten law among botanists that new names shall not be coined in actual reviews and abstracts. In all published papers since Rafinesque's time I know of only one case where this unwritten law has been violated, and the only reason that the binomial *Thea buisanensis* (Sasaki) Metcalf appears in the last Supplement to *Index Kewensis* is that I called the attention of the Kew staff to this strange procedure. Metcalf was reviewing a paper by Sasaki in which the description of *Camellia buisanensis* Sasaki appeared, and de-

[1] Rafinesque, C. S. Remarques botaniques sur quelques plantes de l'Amérique Septentrionale, dans les quatre premiers volumes du *Prodromus* ou *Synopsis plantarum* de de Candolle. *Act. Soc. Linn. Bordeaux*, **6**: 261–269, 1834.

[2] Rafinesque, C. S. [Review of] Flora Philadelphica Prodromus. . . . By Dr. William P. C. Barton. *Am. Month. Mag. Crit. Rev.*, **1**: 356–359, 1817.

——. [Review of] A manual of botany for the Northern States. . . . By Amos Eaton. *Am. Month. Mag. Crit. Rev.*, **1**: 426–430, 1817.

——. [Review of] Flora Americae Septentrionalis. . . . By Frederick Pursh. . . . *Am. Month. Mag. Crit. Rev.*, **2**: 170–176, 265–269, 1818.

——. [Review of] Florula Bostoniensis. . . . By Jacob Bigelow. . . . *Am. Month. Mag. Crit. Rev.*, **2**: 342–344, 1818.

——. [Review of] A sketch of the botany of South Carolina and Georgia. By Stephen Elliott. . . . *Am. Month. Mag. Crit. Rev.*, **3**: 96–101, 1818.

——. [Review of] The Genera of North American Plants. . . . By Thomas Nuttall. . . . *Am. Month. Mag. Crit. Rev.*, **4**: 184–196, 1819.

——. Remarques critiques et synonymiques sur les ouvrages de MM. Pursh, Nuttall, Elliott, Jorrey [= Torrey], Eaton, Bigelow, Barton, Muhlenberg, etc. sur les plantes des États-Unis. *Jour. Phys. Chim. Hist. Nat.*, **89**: 256–262, 1819.

——. Remarks on the Encyclopedia of Plants of Loudon, Lindley, and Sowerby. *Loudon's Gard. Mag.*, **8**: 245–248, 1838; reprinted by Britten, *Jour. Bot.*, **38**: 225–229, 1900, under the title: "An overlooked paper by Rafinesque."

——. American botany, remarks on the Flora of North America by Torrey, Grey [= Gray] and Nuttall. *Good Book:* 37–44, 1840.

liberately made the transfer to *Thea* (see *Lingnan Sci. Jour.*, **12**: 180, 1933). What an opportunity this opens up for the "shufflers" as Rafinesque called them, overlooking the fact that he himself was a past master in the gentle art of changing names in a most unorthodox fashion.

Rafinesque's title is included in the Royal Society's *Catalogue of Scientific Papers,* but is missing from Fitzpatrick's [3] comprehensive bibliography of Rafinesque. In the course of my investigations regarding the particular paper that is the subject of this article, I note several other items that Fitzpatrick apparently overlooked, in spite of the fact that most of them are listed in the *Catalogue of Scientific Papers* (**5**: 75–76, 1871), where a total of forty-three entries are to be found under Rafinesque.

Another reason why Rafinesque did not get along too well with his contemporaries is that, having become familiar with the natural system of classification in Europe, he advocated its adoption in the United States at a time when all our professional botanists were ardent followers of the Linnaean artificial system of classification. Let me quote one violent criticism of the natural system of classification by one of his contemporary American botanists, Amos Eaton, inspired by John Torrey's publication in 1831 of his American edition of Lindley's *Introduction to the Natural System of Botany.* It should be kept in mind that John Torrey was trained in botany by Amos Eaton, and here is a case where the preceptor lagged while his former student forged steadily ahead. In the introduction to the sixth edition of Eaton's *Manual of Botany for North America* (1833),, he states:

> Since Dr. Faustus first exhibited his printed bibles in the year 1463, no book has, probably, excited such consternation and dismay as Dr. Torrey's edition of Lindley's *Introduction to the Natural System of Botany.* And to make the horrors of students, as well as of ordinary teachers, still more appalling, Dr. Torrey's Catalogue of American Plants at the end of his Lindley, was so singularly presented, that it would seem to indicate an awful catastrophe to all previous learning. To relieve all concerned, let me make this pledge: Nothing new is presented either in the text or in the catalogue [*i.e.* his own Manual], excepting what ought to have been discovered in this *progressive* science, since the fifth edition of this Manual was printed; and not so much real improvement has been added as between the fourth and fifth editions. . . . As far as I have any influence I pledge it here, that the embarrassing innovations of De Candolle and others, are of *no possible use* to the science of Botany. . . . An attempt is made by Lindley to prove that the Artificial method of Linnaeus is unnecessary. In doing this he proposes an *Artificial Method* [4] of eleven pages. As those who have not read Torrey's Lindley, will scarcely believe this unaccountable absurdity, they are requested to examine, unbiased, that work between the pages LXVI and LXXX of the introduction. This artificial system is said to lead to the Natural Method. . . . The improvements upon Linnaeus, which have been made, do not authorize any change in the science of Botany other than mere additions and corrections. . . .

It is rather amusing to note Miss McAllister's statement [5] in reference to a published letter written by John Torrey, November 2, 1833, to L. D. von Schweinitz, in that this time Torrey was *more effusive* in his praise of Eaton's *Manual,* quoting: "Have you seen the 6th edn. of Eaton's Manual of Botany? . . . I began to read the preface in a bookstore the other day, & it seemed to be a most remarkable performance." What she did not quote was Torrey's statement that he had seen scarcely more than the covers of the book, as he was interrupted before he had finished the first page; and the first page begins with Eaton's castigation of Torrey, my quoted passage: "Since Dr. Faustus first exhibited his printed bibles in the year 1463, no book has, probably, excited such consternation and dismay as Dr. Torrey's edition of Lindley's *Introduction to the Natural System of Botany.*" No, Torrey's statement regarding Eaton's work as "a most remarkable performance" cannot be interpreted as "effusive praise," but it is manifestly as sarcastic and ironic as a gentle soul like John Torrey cared to be.

It will be noted, from the data given below, that Rafinesque was not only encyclopedic in his writings, as to subject matter, but also most diffuse as to places of publication. In his own *Life of Travels* (1836: 8–9) he says regarding his education:

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[3] Fitzpatrick, T. J. *Rafinesque. A sketch of his life with bibliography.* Des Moines: 1–241, 32 pl., 1911.

[4] This is scarcely true, for what is presented is an *artificial analysis* of the orders, in the form of a key to the classes (Vasculares, Cellulares), subclasses (Exogenae or dicotyledonous plants, and Endogenae or monocotyledonous plants), tribes (Angiospermae, Gymnospermae, Petaloideae, and Glumaceae), and to the families under each division and subdivision, these families, as to limits (but naturally not as to sequence, as at present understood), much the same as they stand today.

[5] McAllister, Ethel M. *Amos Eaton, scientist and educator.* Philadelphia: xiii + 587, 8 pl., 1941 (p. 235).

I never was in a regular College, nor lost my time on dead languages; but I spent it in learning alone and by mere reading ten times more than is taught in Schools. I have undertaken to learn the Latin and Greek, as well as the Hebrew, Sanscrit, Chinese and fifty other languages, as I felt the need or the inclination to study them.

How close Rafinesque was to the modern and generally accepted principles of evolution is indicated by the following quotation from his *Herbarium Rafinesquianum* (1833: 11–12), where he expresses his principles of philosophy in reference to new genera and new species of plants and animals. It will be noted that this sounds "very modern" indeed:

Extract of a letter to Dr. J. Torrey of New York dated 1st Dec. 1832:—I shall soon come out with my avowed principles about G. and Sp. partly announced 1814 in my principles of Somiology, and which my experience and researches ever since have confirmed. *The truth is that Species and perhaps Genera also, are forming in organized beings* by gradual deviations of shapes, forms and organs, taking place in the lapse of time. There is a tendency to deviations and mutations through plants and animals by gradual steps at remote irregular periods. This is a part of the great universal law of PERPETUAL MUTABILITY in every thing.

Thus it is needless to dispute and differ about new G. Sp. and varieties. Every variety is a deviation which becomes a Sp. as soon as it is permanent by reproduction. Deviations in essential organs may thus gradually become N.G. Yet every deviation in form ought to have a peculiar name, it is better to have only a generic and specific name for it than 4 when deemed a variety. It is not impossible to ascertain the primitive Sp. that have produced all the actual; many means exist to ascertain it: history, locality, abundance &c. This view of the subject will settle botany and zoölogy in a new way and greatly simplify those sciences. The races, breeds or varieties of men, monkeys, dogs, roses, apples, wheat . . . and almost every other genus, may be reduced to one or a few primitive Sp. yet admit of several actual Sp. names may and will multiply as they do in geography and history by time and changes, but they will be reducible to a better classification by a kind of genealogical order or tables.

My last work on Botany if I live and after publishing all my N. Sp. will be on this, and the reduction of our Flora from 8000 to 1200 or 1500 primitive Sp. with genealogical tables of the gradual deviations having formed our actual Sp. If I cannot perform this give me credit for it, and do it yourself upon the plan I trace. C. S. R.

There is no doubt as to Rafinesque's versatility, nor as to the brilliance of his mind, yet at the same time he was most erratic. Weiss [6] does not overstate the case in the following quoted passage:

Rafinesque has been described as an eccentric, absent-minded person, whose classrooms were places of easy behavior. Some of his communications were rejected by the Academy of Natural Sciences as "wild effusions." His scientific weaknesses have been exposed and described in uncomplimentary adjectives. He did not always give credit to the sources of his information, and this displeased his contemporaries. He started many things, only to abandon them. He was distrustful, and blamed his failures on "secret foes." He allowed his imagination to run wild. He was a genius. He was egotistical. He was erratic. He did not fit into accepted patterns. And yet in spite of all, he accomplished much that will remain as long as natural history is studied.

Regarding his biological work Rafinesque himself says: [7]

As I think that I am gifted with a peculiar sharp sagacity in discriminating Genera and Species of Plants and Animals, it behooves me to use it in order to rectify these objects and the sciences relating thereto. It is what I have often done, am now doing, and will continue to do as long as I live, not being prevented by the sneer or neglect of any one whom I consider less sagacious than myself, who cannot discriminate between the most conspicuous characters blended by the Linneists or modern Blenders and Schufflers.

And as to his own belief in himself, note the next to the last paragraph in his own *Life of Travels* (1836):

Versatility of talents and of professions, is not uncommon in America; but those which I have exhibited in these few pages, may appear to exceed belief; and yet it is a positive fact that in knowledge I have been a Botanist, Naturalist, Geologist, Geographer, Historian, Poet, Philosopher, Philologist, Economist, Philanthropist. . . . By profession a Traveller, Merchant, Manufacturer, Collector, Improver, Professor, Teacher, Surveyor, Draftsman, Architect, Engineer, Pulmist, Author, Editor, Bookseller, Librarian, Secretary . . . and I hardly know myself what I may not become as yet; since whenever I apply myself to anything, *which I like*, I never fail to succeed if depending in me alone, unless impeded and prevented by lack of means, or the hostility of the foes of mankind.

[6] Weiss, H. B. *Rafinesque's Kentucky friends.* Highland Park, N. J.: 18, 1936.

[7] Rafinesque, C. S. *New flora of North America,* 4: 6, 1838.

Just how impracticable Rafinesque could be at times is well illustrated by his remarkable letter to Dr. Charles W. Short, February 22, 1822, reproduced by Perkins.[8] Apparently dissatisfied with his position in Transylvania University, and learning of a proposition to establish the Western College of Kentucky in Hopkinsville, he urged Dr. Short to use his influence with a view to offering him the presidency of this proposed new institution. He states:

> I conceive that 50 students could easily be procured to begin with, from your town and neighborhood (even young ladies might be admitted as in Cincinnati in the beginning) which at $40.00 each per year (tuition is now $50.00 in our Univ'y) tuition would produce $2000.00 upon which I calculate that a start might be made. I would only require for me from $1000. to $1500. say $1200. to be increased if more pupils are rec'd and two Professors or tutors might be procured from our University for $400 or $500 each yearly, to teach the grammar, the latin and greek languages, mathematics, and otherwise help me in my branches, which would be all the arts and Sciences, literature, History, Drawing, the French & Spanish languages &c.

Rafinesque's offer included his library of 1000 volumes, an herbarium of 15,000 specimens, and a cabinet or museum of 5000 specimens of minerals, fossils, shells, insects, animals, etc. Mr. Perkins' comment on this is short and to the point, closing with this statement: "In other words, the $800.00 left over after Rafinesque's yearly salary might be used, he suggested, to pay two professors and the running expenses of the institution for a year. What an astonishing offer! No wonder it was not accepted."

Other evidence of his impracticability was his persistence in publishing, at his own expense, an extraordinary number of books, pamphlets, and periodicals, between the years 1803 and his death in 1840, other than the numerous papers that he published in various American and European serials. For most of his independently published technical works there could have been only a very limited demand in the United States at the time they appeared. There were few botanists and zoölogists, and few institutions that sponsored this type of investigation at the time Rafinesque was active. Rafinesque realized this, for he states[9] regarding his *Flora Telluriana:* "Only 160 copies were printed, which makes it high, as but few copies can be sold in America, where Botanists cannot duly appreciate it, and they must be sent to Europe, to be often exchanged instead of sold." Imagine the persistence of an individual, living in Philadelphia in the decade between 1830 and 1840, who had the courage of his convictions, and prepared and published a *Flora* and a *Sylva* of the World! He lived penuriously, devoting every penny and every dollar that he could save from his income, which never could have been very large, to the advancement of his natural history and other work, and to the actual publication of his writings. It is pathetic to note how little encouragement he received from others. In 1821, when he proposed to publish in Lexington, Kentucky, by subscription at $1.00 per volume, a selection of his miscellaneous works and essays, he met with very little success, for the copy of his prospectus in the Library of the Museum of Comparative Zoölogy at Harvard University is apparently the one that he used in canvassing, and this bears the signatures of only eight subscribers! The proposed publication was never issued. Even today, with the vastly increased interest in natural history, with its many thousands of devotees and hundreds of institutions and libraries concerned more or less with the type of technical literature that Rafinesque sponsored, it is very doubtful if receipts from sales would actually cover publication costs had these works been issued a century later.

We should, however, take exception to the current gross exaggeration regarding Rafinesque's paper on lightning (*Western Rev.,* 1: 60–62, 1819). Regarding this Fitzpatrick states:

> A popular account of the lightning with descriptions of the various shapes which the flash may assume. This article is the basis of the gross calumny inflicted upon Rafinesque by various writers. They only discredit themselves who charge that Rafinesque deliberately described twelve new species of thunder and lightning.

Yet this is one of the myths that is repeated over and over again. One has only to read the article to realize how false the accusation is.

Botanists are apt to think of Rafinesque primarily as a botanist, and zoölogists tend to consider him as a zoölogist; he was essentially *a naturalist.* His published papers appertain not only to such diverse phases of natural history as botany, horticulture, ornithology, icthyology, mammalogy, entomology, conchology, palaeontology,

[8] Perkins, S. E., III. Letters by Rafinesque to Dr. Short in the Filson Club Archives. *Filson Club Hist. Quart.,* 12: 200–239, 1938.

[9] Rafinesque, C. S. *Flora Telluriana,* 4: 4, 1838.

geology, mineralogy, etc., but also to such wide-ranging subjects as geography, meteorology, agriculture, medicine, materia medica, pharmacology, chemistry, metaphysics, political economy, statistics, history, art, commerce, mathematics, legislation, education, banking, ethics, comparative philology, astronomy, archaeology, travel, philosophy, phrenology, Free Masonry, and various other subjects. He even dabbled in literature, writing both prose and poetry. This very diffuseness of publication was undoubtedly another factor that discredited Rafinesque among his contemporaries.

Under these circumstances naturally a *complete* bibliography is an exceedingly difficult task, and the surprising thing is that Fitzpatrick located so much, listing as he does about nine hundred and forty titles,[10] not including numerous unpublished manuscripts. The situation is further complicated by Rafinesque's listing in his various publications numerous titles of articles that he proposed to publish. Some of these were unquestionably in manuscript form, but others were probably never written. In many cases he gives the titles of manuscripts that he sent to various individuals and organizations for publication—manuscripts that were lost, declined, or merely ignored by those who received them. The following may be added to Fitzpatrick's bibliography of Rafinesque:

Rafinesque, C. S. Ueber eilf neue Sippen von Mollusken, ausgestellt 1814. *Isis von Oken* 1820, **1**: Lit. Anz., 244–247, 1820.

This is a German translation of Fitzpatrick's item no. 301: "Descriptions de onze genres nouveaux de mollusques, publiés en 1814." *Jour. Phys.*, **89**: 150–154, 1819.

——. Natürliche Verwandschaften zwischen den Sippen *Viscum, Samolus* und *Viburnum*. *Isis von Oken* 1821, **2**: 978–979, 1821.

This is a German translation of Fitzpatrick's no. 345: "Remarques sur les rapports naturels des genres *Viscum, Samolus* et *Viburnum*." *Ann. Gén. Sci. Phys.*, **5**: 348–351, 1820.

——. Beschreibung und natürliche Classification der *Floerkea*. *Isis von Oken* 1822, **2**: 1319–1321, 1822.

This is a German translation of Fitzpatrick's no. 284: "Description and natural classification of the genus *Floerkea*." *Am. Jour. Sci.*, **1**: 373–376, 1819.

——. Drei neue Pflanzensippen aus dem Staate New-York. *Isis von Oken* 1822, **2**: 1321–1323, 1822.

A German translation of Fitzpatrick's item no. 285: "Descriptions of three new genera of plants from the State of New-York." *Am. Jour. Sci.*, **1**: 377–379, 1819.

——. Über Myosurus Shortii. *Isis von Oken* 1822, **2**: 1322, 1822.

A German translation of Fitzpatrick's no. 286: "Notice on the *Myosurus Shortii*." *Am. Jour. Sci.*, **1**: 379–380, 1819.

——. Remarques botaniques sur quelques plantes de l'Amérique Septentrionale, dans les quatre premiers volumes du Prodromus ou Synopsis plantarum de de Candolle. *Act. Soc. Linn. Bordeaux*, **6**: 261–269, 1834.

——. Sur les fossiles de la vallée Sherman des monts Alleghany. *Bull. Soc. Géol. France*, **10**: 381–383, 1839.

——. Descriptions des genres fossiles *Ditaxopus, Trianisitis, Troxites, Menepites*, et *Trianistes*. *Bull. Soc. Géol. France*, **10**: 378–381, 1839.

——. Neogenyton, or indication of sixty-six new genera of plants of North America: 1–4, 1825.

No. 1 of a series of reprints of rare classical works of natural history, issued by the *American Midland Naturalist*, July, 1912. Supplementary to Fitzpatrick's no. 474.

——. Monographie des coquilles bivalves et fluviatiles de la rivière Ohio. Remarques sur les rapports naturels des genres *Viscum, Samolus* et *Viburnum*. A Bruxelles, de l'Imprimerie de Weissenbruch père, Rue du Musée, No. 1057. 1820 [part of *Annals of Nature*] pp. 17 to 60 (41 pages).

No. 2 of a series of reprints of rare classical works of natural history, issued by the *American Midland Naturalist*, July, 1912. The title is as printed in that journal. The title-page, second page, and three plates are said to be facsimile reproductions, the text a word-for-word reprint. This is supplementary to Fitzpatrick's Nos. 344, 345, 363, 934, the originals of both papers included being in the *Ann. Gén. Sci. Phys.*, **5**: 287–322, 348–351, 1820.

——. 10. Scadiography of 100 genera of ombelliferous plants, etc.: 49–61, 1840.

No. 3 of a series of reprints of rare classical works of natural history, issued by the *American Midland Naturalist*, July, 1913. This is a reprint of pp. 49–61 from Rafinesque's *Good Book*. Supplementary to Fitzpatrick's no. 911.

——. 5. Botany. The natural family of Carexides: 23–28, 1840.

No. 4 of a series of reprints of rare classical works of natural history, issued by the *American Midland Naturalist*, July, 1913. This is a reprint of pp. 23–28 from Rafinesque's *Good Book*. Supplementary to Fitzpatrick's no. 906.

——. Autikon Botanikon: 1–200, 1840.

A facsimile lithoprint reproduction of this work, July, 1942, Arnold Arboretum, Jamaica Plain, Mass. Supplementary to Fitzpatrick's nos. 897, 898, 899.

[10] This is a false total, as Rafinesque's different titles are considerably less than this number. Fitzpatrick numbered each part of the continued works separately, such as the three parts of the *Autikon Botanikon*, which form one continuously paged volume, the several parts of the *Medical Flora*, the *Flora Telluriana*, and the *New Flora and Botany of North America*, while the titles of Rafinesque's various serials are numbered independently of the titles of the papers included in each. The listing of the separate parts of *The School of Flora* between nos. 476 and 604 as 96 separate titles is pure padding. The number of really distinct papers should be reduced by well over a hundred. A certain number are reprints of Rafinesque's papers that were issued by various individuals long after his death.

To Fitzpatrick's *Bibliotheca Rafinesquiana* (pp. 223–239), may be added:

Anonymous. The *Lexington Herald:* Body of Rafinesque, famous scientist, is brought to Transylvania College. March 1, 1924. *Medical Life,* **31**: 155–158, 1924.

——. Constantin Rafinesque. *Missouri Bot. Gard. Bull.,* **15**: 164–171, 1927.

——. Celebrated conservationists and naturalists in our National Parks. Constantin Samuel Rafinesque. U. S. Dept. Interior, press release: 1–11, 1938.

Barkley, A. H. Constantin Samuel Rafinesque. *Ann. Med. Hist.,* **10**: 66–76, 2 portr., 1928.

Barnhart, J. H. Brief sketches of some collectors of specimens in the Barton Herbarium. *Bartonia,* **9**: 35–42, 1926 (Rafinesque, p. 41).

Chase, A. The Durand Herbarium. *Bartonia,* **17**: 40–45, 1936.

On page 44 Mrs. Chase notes that Durand preserved none of Rafinesque's type specimens of the grasses.

Coulter, J. M., and Rose, J. N. Musineon of Rafinesque. *Bot. Gaz.,* **20**: 258–260, 1895.

Coville, F. V. The technical name of the camas plant. *Proc. Biol. Soc. Washington,* **11**: 61–65, 1897.

Fernald, M. L. Some genera and species of Rafinesque. *Rhodora,* **34**: 21–28, figs. 1–6, 1932.

Haag, H. B. Rafinesque's interests—a century later; medicinal plants. *Science,* n.s., **94**: 403–406, 1941.

Hance, A. M. Rafinesque; the great naturalist. A paper read before the Bucks County Historical Society at Langhorne [Pa.], June 4th, 1914. 1–12, 1914.

Harrison, I. W. The Transylvania Botanic Garden. A little-known American enterprise of great historic, scientific, and educational interest. A study of "The Athens of the West"—Lexington, Kentucky. Home of the first printing press, newspaper, public library, and university, west of the Alleghanies. A pioneer naturalist of a century ago [Constantin Samuel Rafinesque] and the botanic garden he sought to found. *Jour. Am. Hist.,* **7**: 901–909, 1913.

Heck, E. L. W. Constantine Rafinesque. *Sci. Monthly,* **25**: 554–558, 1927. (Reprint: 1–5.)

Jordan, D. S. The bones of Rafinesque. *Science,* n.s., **59**: 553–554, 1924.

Leonard, W. E. Some early Philadelphia botanists; Schweinitz, Nuttall, Rafinesque and Darlington. *Bull. Minnesota Acad. Nat. Sci.,* **3**: 29–37, 1889. (Rafinesque, pp. 31–35.)

Merrill, E. D. Modern facsimile reproductions of rare technical publications. *Science,* n.s., **96**: 180–181, 1942.

Pennell, F. W. "Unrecorded" genera of Rafinesque. —1. Autikon Botanikon (1840). *Bull. Torrey Bot. Club,* **48**: 89–96, 1921.

——. Elias Durand and his association with the Academy of Natural Sciences of Philadelphia. *Bartonia,* **17**: 33–39, 1936.

Includes important data regarding the fate of Rafinesque's herbarium.

——. New light on Rafinesque. *Chron. Bot.,* **6**: 125–126, 1940.

——. The life and work of Rafinesque. An address delivered at Transylvania College, Lexington, Kentucky, at the centennial held in Rafinesque's honor on October 30, 1940. Lexington: 1942 (in press).

——. Botanical collectors of the Philadelphia local area. *Bartonia,* **21**: 38–57, 1942.

Includes very important data regarding Rafinesque's herbarium, pp. 50–55.

Perkins, S. E., III. Letters by Rafinesque to Dr. Short in the Filson Club Archives. *Filson Club Hist. Quart.,* **12**: 200–239, 1938.

Rehder, A. Note on *Basilima* and *Schizonotus* of Rafinesque. *Bot. Gaz.,* **32**: 56–58, 1901.

Rhoads, S. N. Rafinesque as an ornithologist. *Cassinia,* **15**: 1–12, portr., (1911) 1912.

——. Additions to the known ornithological writings of C. S. Rafinesque. *Auk,* **29**: 191–198, 1912.

Includes reproductions of two papers by Rafinesque published in the *Kentucky Gazette* in 1822, under the subtitle of *The Cosmonist,* nos. 3 and 4, with notes.

——. Ornithological notes of Rafinesque in the *Western Review and Miscellaneous Magazine,* Lexington, Ky. *Auk,* **29**: 401, 1912.

Brief abstracts of Rafinesque's notes on birds and meteorological subjects.

Rodgers, A. D., III. John Torrey, a story of North American botany. Princeton: 1–352, 1 portr., 1 map, 1942.

Rafinesque, pp. 16, 22, 24–25, 29–34, 36, 38, 68, 89, 99, 107–108, 148, 229.

Smyth, S. G. Rafinesque. The errant naturalist. *Hist. Soc. Montgomery County* [Pa.] *Historical Sketches,* **6**: 300–341, 1 pl., 1929.

Sprengel, K. Florula Ludoviciana 1817. *Jahrb. Gewächsk.,* **1**(2): 165–166, 1819.

——. Constant. Sam. Rafinesque's neueste Entdeckungen. In his: *Neue Entdeck.,* **1**: 142–146, 1820.

Steudel, E. G. Ueber C. F. [sic!] Rafinesque in Philadelphia literarische Arbeiten und Tausch-Anerbietungen. *Flora,* **3**(1), Beil., **2**: 33–42, 1820.

Tornabene, F. Quadro storico della botanica in Sicilia che serve di prolusione all' anno scolastico 1846 e 1847 nella Regia Università degli studi in Catania: 1–70, 1847.

Includes a brief summary of Rafinesque's published work on the flora of Sicily, pp. 50–51, with data regarding his proposed but never consummated publication of Cupani's plates of the *Ponphyton Siculum* (Pritzel, no. 1995); see Fitzpatrick's item no. 23.

Weiss, H. B. Rafinesque's Kentucky friends. Highland Park, N. J.: 1–70, 25 pl., 1936.

The illustrations are reproductions of Rafinesque's sketches of various individuals he knew, chiefly residents of Kentucky, the originals in the library of Transylvania College, Lexington, Kentucky.

Welden, L. von. Rafinesque Entdeckung neuer Gewäsche im Schwimmen. *Flora,* **5**(2): 719, 1822.

This very curious title appears only in the Inhaltsverzeichniss in the Sechste Beilage, page 86. In the text the article appears under the heading "Curiosa."

Whaler, J. Green River, a poem for Rafinesque. New York: 1–153, 1931.

Rafinesque's Publications in American Periodicals

As one scans a chronological list of Rafinesque's published papers, one notes immediately that, as

far as publication in periodical literature is concerned, the period of acceptance for publication in any one serial is always short, and this applies to both the American and the European periodicals to which he sent manuscripts. Some of the American periodicals, with the years in which Rafinesque papers appear, are: *The Medical Repository,* 1804; *The Philadelphia Medical and Physical Journal,* 1805; *The American Monthly Magazine and Critical Review,* 1817–19; *The American Journal of Science and Arts* (*Silliman's Journal*), 1818–21; *The Western Review and Miscellaneous Magazine,* 1819–21; *The Kentucky Gazette,* 1821–22; *The Cincinnati Literary Gazette,* 1824; *The Saturday Evening Post* (*Atkinson's Post*), 1826–31; *The Casket, or Flowers of Literature, Wit and Sentiment* (*Atkinson's Casket*), 1826–31; and *The Monthly American Journal of Geology and Natural Science,* 1832.

Doubtless the reason why Rafinesque did not continue to contribute to any single periodical for more than a few years, is indicated in a footnote to the following paragraph in Asa Gray's essentially fair summarization of Rafinesque's botanical work:[11]

> A gradual deterioration will be observed in Rafinesque's botanical writings from 1819 to about 1830, when the passion for establishing new genera and species appears to have become a complete *monomania.* This is the most charitable supposition we can entertain, and is confirmed by the opinions of those who knew him best.

The footnote from this passage by the editor of the Journal (Benjamin Silliman) is as follows:

> It was in this year (1819) that I became alarmed by a flood of communications announcing new discoveries by C. S. Rafinesque, and being warned, both at home and abroad, against his claims, I returned him a large bundle of memoirs, prepared with his beautiful and exact chirography, and in the neatest form of scientific papers. This will account for the early disappearance of his communications from this Journal. The step was painful but necessary, for if there had been no other difficulty, he alone would have filled the Journal, had he been permitted to proceed.

We can only assume that editors of other periodicals found it necessary to take similar action.

It will be noted that many of the American periodicals in which Rafinesque published are not strictly the type in which later generations of taxonomists would expect to find systematic papers. Complete sets of some of these are rare in American libraries, and if American scientists are handicapped in gaining access to some of Rafinesque's papers, how much more seriously handicapped are our European colleagues! Still it should be remembered that in Rafinesque's time there were no American botanical periodicals, the first one in North America, the *Bulletin of the Torrey Botanical Club,* not being established, and then only on a very modest scale, until 1870, and the second, *The Botanical Gazette,* on an equally modest scale five years later.

Rafinesque's Publications in European Periodicals

If Rafinesque scattered his contributions to American serials, to the vexation of most scientists who must occasionally consult them, he was equally diffuse in his sendings to European periodicals; and acceptance or rejection of his papers there apparently followed the American pattern mentioned above. Some of these foreign periodicals are: *Bulletin des Sciences de la Société Philomatique de Paris* (1803); *Journal de Botanique,* Paris (1808–09); *Journal de Botanique* (Desvaux) (1813–14); *The Philosophical Magazine and Journal,* London (1819); *Journal de Physique, de Chimie et d'Histoire Naturelle, et des Arts,* Paris (1819–20); *Isis von Oken* and its supplement, the *Litterarischer Anzeiger*[12] (1819–21); *The Quarterly Journal of Science, Literature and Arts,* London (1820); *Annales Générales des Sciences Physiques,* Brussels (1820–21); *The Gardener's Magazine and Register of Rural and Domestic Improvement,* London (1832); *Actes de la Société Linnéenne de Bordeaux* (1834); *Bulletin de la Société Géologique de France,* Paris (1839); and *Journal of the Royal Geographical Society of London* (1841). As many of the American periodicals favored with Rafinesque's contributions are not generally available to European scientists, so also, most of the European periodicals listed above were, and in many cases still are, not generally accessible to their American colleagues.

Rafinesque's Private Periodicals

A third and interesting category of Rafinesque's serial publications comprises those periodicals, all

[11] Gray, A. Notice of the botanical writings of the late C. S. Rafinesque. *Am. Jour. Sci.,* **40**: 221–241, 1841.

[12] This is usually cited by Fitzpatrick merely as *Litterarischer Anzeiger.* This forms a separately paged part of certain volumes of *Isis von Oken* as *Litterarischer Anzeiger sur Isis* or merely as *Litterarischer Anzeiger.*

short-lived, that he established, edited, and published, such as the *Specchio delle Scienze o Giornale Enciclopedico di Sicilia* (1814); *The Western Minerva; or American Annals of Knowledge and Literature* (1820); *Annals of Nature* (1820); *The Atlantic Journal and Friend of Knowledge* (1832–33); the *Herbarium Rafinesquianum* (1833); the *Bulletin of the Historical and Natural Sciences* (1836–39); and *The Good Book and Amenities of Nature* (1840); most of these did not get beyond volume one, and some ceased with number one. These were all projected as periodicals. In addition to his own short-lived serials and the numerous papers published by him in various foreign and domestic periodicals, he independently published a great many pamphlets and books, and some of these items are now among the rarest of botanical papers, existing in only a very few libraries. It is worthy of note that in the second edition of his *Thesaurus* (1871) Pritzel admitted only four of Rafinesque's works, although in the first edition (1851) he listed fifteen titles. This probably reflects the poverty of European botanical libraries in Rafinesque's publications.

Rafinesque's Overlooked Names

One result of Rafinesque's publication methods has been most unfortunate, in that some of his generic names and very many of his published binomials are not listed in any botanical indices to date. Thus eighty-three new generic names and seventy new binomials published by Rafinesque in his *Autikon Botanikon* in 1840 were not listed in *Index Kewensis* until the seventh supplement appeared in 1929, and then the entries were made from Pennell's paper, "'Unrecorded' Genera of Rafinesque" (*Bull. Torrey Bot. Club*, **48**: 89–96, 1921); there was no copy of the *Autikon Botanikon* at Kew. Incidentally there are *several hundred* validly published binomials in this work that are still not included in *Index Kewensis*. The numerous new names in several other volumes and some of Rafinesque's scattered small papers still remain to be listed. It is suspected that these overlooked and unrecorded generic and specific names, published in 1840 and earlier, may exceed one thousand and possibly approximate fifteen hundred. Because of the homonym rule, if for no other reason, it is highly desirable that these names be listed, and I personally think that after a lapse of over a hundred years it is high time that this task was accomplished; I plan to prepare and publish such a list.

A Review of American Private Botanical Periodicals

In the establishment of privately published, short-lived technical periodicals, consisting solely of papers prepared by the proprietor-editor-publisher, Rafinesque was apparently the pioneer in his *Specchio delle Scienze* (1814), *Annals of Nature* (1820), and others mentioned above, for he had an overwhelming urge to publish. In this particular field he had no followers in the United States until toward the end of the nineteenth century, when the publication urge again became strong with various botanists, and the idea burgeoned anew and with a diversified assortment of titles, in printing good, bad, and indifferent. This American botanical effort is summarized below. In this recrudescence Greene, characterized by Underwood as Rafinesque's one real successor, like Rafinesque, changed his titles at intervals, including *Pittonia*, **1–5** (1887–1905), *Leaflets of Botanical Observation and Criticism*, **1–2** (1903–12), and *Cybele Columbiana* (1914) (only volume one, number one, issued). But we may also list the Brandegee-Eastwood-Brandegee *Zoe*, **1–5** (1890–1908), the Orcutt *West American Scientist*, **1–22** (1884–1919), the Orcutt *American Botanist*, **1** (1, 2) (1898–1900), the Orcutt *American Plants*, **1–3** (1907–12), the Orcutt *Orcutt*, **1** (1 only) (1897),[13] the Jepson *Erythrea*, **1–7** (1893–99) plus **8**, nos. 1–13 (1922 and 1938), the Heller and Heller-Kennedy *Muhlenbergia*, **1–9** (1909–1915), the Kellermann *Journal of Mycology*, **1–14** (1883–1915), the Fitzpatrick *Iowa Naturalist*, **1–3** (1905–11), the Bailey *Gentes Herbarum*, **1–5** (1920–42), the Suksdorf *Werdenda*, **1** (1–8) (1923–31), the Blanchard *Betula*, **1** (1, 2) (1904), the Jones *Contributions to Western Botany*, **1–18** (1891–1935) (in part consisting of reprints from other periodicals), the Ames *Schedulae Orchidaceae*, **1–10** (1922–31), the Elmer *Leaflets of Philippine Botany*, **1–10** (1906–1939), the Eastwood-Howell *Leaflets of Western Botany*, **1–3** (1932–42), the Gleason-Moldenke *Phytologia*, **1–2** (1933–42), the Clute *American Botanist*, **1–48** (1901–1942), the Clute *Fern Bulletin*, **1–20** (1893–1912) (volumes **1–4** as the *Linnaean Fern Bulletin*), *The Asa Gray Bulletin*, **1–8** (1893–

[13] The only part of this issued, to my knowledge, is the very poorly printed number one, consisting of twelve pages "price 15 cents, issued weekly, $5.00 a year in advance," with a proposition for life subscriptions at $50.00! The one part issued is of no botanical or other value and is worthy of mention here only as a bibliographical curiosity because of its extraordinary title.

1900), the Grout-Smith *The Bryologist,* **1** (1898) to **14** (1911), after which it was taken over officially by the Sullivant Moss Society and is now in its forty-fifth volume, the Knowlton-Pollard-Lloyd-Shreve *Plant World,* **1–22** (1897–1919), the Gleason-Fulling *Botanical Review,* **1** (1935) **–8** (1942), and even *Lorquinia,* **1–2** (1916–1917). An abortive attempt to establish a periodical is perhaps represented by the Ashe *Botanical Contributions from my Herbarium;* one wonders just why the redundant word "Botanical" was included. No. 1 was issued privately October 28, 1897, the title of the one article being: "The genus *Asarum* in Eastern America." The paper was apparently written for the *Journal of the Elisha Mitchell Scientific Society,* but no explanation is given of its earlier issue as an independent publication. It appears, with some eliminations and emendations in that journal (**14**: 31–36, 1897) as: "The glabrous leaved species of *Asarum* of the Southern United States," without the "Botanical Contributions" part of the title. The series was continued in the form of reprints from various periodicals as *Contributions from my Herbarium.* The Smith *Species Lupinorum* (1938–1942), twenty signatures, must, I believe, be considered as a periodical. It is privately published (offset reproduction of typed copy, like *Phytologia*) in Saratoga, Calif. What may prove to be even another privately published serial is advertised by the same author (Charles Piper Smith) at the bottom of page 304 of signature nineteen, September, 1941, as: "Two signatures of 'Geranium Records' are also available"; I have seen no copy of this. Perhaps the most extraordinary of all of these is the very recent Davis *Nature Leaflet,* number one, 1941, presumably published in Brownsville, Texas.[14] The *Mycological Writings of C. G. Lloyd,* **1–7** (1898–1925), probably belongs in this category as it was issued during the period that the author operated the Lloyd Library, which in 1938 established a standard quarterly periodical under the name of *Lloydia.* The Tidestrom *Elysium Marianum* **[1–3]** (1906–10) is perhaps to be considered rather as small individual volumes than as a "periodical."[15]

As to names, *Werdenda* is perhaps the most curious, being a hybrid of German-Latin construction. Suksdorf had encountered difficulties in getting his papers, written wholly in German, accepted for publication in German and American periodicals, and solved his difficulties by establishing his own medium of publication. He explains the name *Werdenda* thus: "Der Name *'Werdenda'* soll dieses andeuten: zugleich ist damit auch die Gegenwart gemeint, den diese ist der Zeitraum oder der Augenblick des Werdens[16] und des Wachsens. Eine der Nornen heisst *Werdendi,* die Gegenwart." Comment on Orcutt's peculiar title for his abortive journal *Orcutt* is superfluous.

In some of these periodicals, particularly those of Jones and Orcutt, the printing leaves very much to be desired, being not unlike some of Rafinesque's attempts at private publication, but in others

[14] This very recent attempt is in a class by itself, and violates most accepted principles of technical publication, in that it is unpaged and bears no place of publication. It is a small folder, 15.5 × 9.5 cm., consisting of a title-page and a page and a half of text containing the technical description of *Verbena cameronensis* sp. nov. by L. Irby Smith. It was issued August 15, 1941, with a "second printing" August, 1941, "correcting the errors in the first." At the bottom of the title-page is the imprint "Lower Rio Grande Valley Nature Club." Its price is given as five cents. Regarding it, Dr. W. H. Camp (*Taxonomic Index,* **4**: entry 305, 1941) says: "Because of certain Post Office laws, our opinion of such media for publication of new species cannot be expressed here."

[15] Contemporaneous with and immediately following Rafinesque is a curious series of titles, some of which, on occasion, were considered as botanical, although in them botany is conspicuous largely by its absence. They are for the most part periodicals devoted to the support of the Thomsonian system of medicine, a system that quickly gained its apogee and as quickly declined. One of the doctrines was that as all minerals are from the earth, their use tends to send men to their graves; ergo, if one uses mineral drugs, one dies early, but if one uses herb remedies, one presumably might live forever, or at least greatly prolong one's life, as herbs grow upward. This phenomenon is mentioned here in passing because not infrequently one finds runs of these magazines in botanical libraries, because trusting and not too well-informed librarians and even directors of botanical institutions have been beguiled into purchasing such sets from the titles. Dr. Samuel Thompson of Massachusetts lived from 1769 to 1843, and I note that as late as 1889 this statement is made: "Even to this day *Thomsonianism* has its votaries, and lobelia and rum sweats are retained with the tenacity of old friends" (*Pop. Sci. News,* **23**: 61, 1889). Among the titles are the *Botanic Investigator,* **1** (1–5) (1835), *Botanic Journal,* **1** (1–11) (1836–37), *Botanic Luminary,* **1–2** (1836–38), *Botanic Watchman,* **1–2** (1834–35) (*The sun of science arising upon the flora of North America*), *Botanic Advocate,* **1** (1–11) (1835–36), *Botanic Advertiser,* **1–5** (1839–40), *Botanic Sentinel,* **1–8** (1835–44), with various others published here and there in Poughkeepsie, Boston, Maryville, Norwich, Burlington, Manchester, Cincinnati, Philadelphia, and probably in other centers as "Thomsonian" periodicals but without the modifying term "Botanic." The period covered is from about 1834 to 1840, a few persisting until 1844.

[16] Werden to become, grow; to be created; to happen or occur.

it is excellent, there being all shades of gradation from atrociously bad to thoroughly good. Incidentally, the language in some of these privately operated periodicals is distinctly robust when it comes to the expression of frank opinions on the part of certain of these author-editor-proprietor-publishers regarding details of the work of their associates and contemporaries, in some cases sinking to the level of personal vituperation. I am somehow reminded of President Lowell's comment, when he had to settle some problem that arose among certain *prima donna* botanists at Harvard: "What is it about the pretty little flowers that make the botanists quarrel so much among themselves?" And here I feel inclined to interpolate a quotation from Bret Harte:

> Now, I hold it is not decent for a scientific gent
> To say another is an ass—at least, to all intent;
> Nor should the individual who happens to be meant
> Reply by heaving rocks at him to any great extent.

Occasionally one can smile at an editor-author-proprietor's expressed opinion of himself, for I have always been intrigued by this gem:

> Come to think of it, this magazine [*The Fern Bulletin*] and the *American Botanist* [also published by the same individual] are about the only two botanical publications that can boast an editor. The so called editors on [sic!] most of the others are mere proof readers and copy holders—sort of elevated office boys. (*Fern Bull.*, **19**: 24, 1911.)

It will be noted that most of these hopefully established "personal" periodicals are western in origin, but the east held the record for brevity in Blanchard's *Betula* (1904) that consists of volume one, numbers one and two, with a total of three printed pages, until the even more abbreviated Davis *Nature Leaflet* appeared in 1941. While I have confined my attention to the American contributions to this type of private botanical serials, the phenomenon is by no means confined to the United States nor to botany.

Early European Private Botanical Periodicals

In any discussion of privately published botanical periodicals it is only fair to note that in the beginning of botanical serial publications the earliest ones were all privately sponsored. As these were more or less in advance of their times, the mortality was great, for few persisted for more than one to three volumes. Among the pioneer botanical serials may be mentioned the Usteri *Magazin für die Botanik—Annalen der Botanik —Neue Annalen der Botanik*, **1–24** (1787–97), the Römer *Neues Magazin für die Botanik*, **1** (1794), the *Botanical Review, or the Beauties of Flora*, **1** (1789–90), the Römer *Archiv für die Botanik*, **1–3** (1796–1803), the Schrader *Journal für die Botanik*, **1–5** (1799–1803), the Schrader *Neues Journal für die Botanique*, **1–4** (1806–10), the Konig and Sims *Annals of Botany*, **1–2** (1805–06), the *Journal de botanique* (Desvaux), **1–4** (1813–14), and its predecessor *Journal de botanique* (Paris), **1–2** (1808–09), the Sprengel, Schrader, and Link *Jahrbücher für Gewächskunde*, **1–3** (in one) 1818–20, the Guillemin *Archives de Botanique*, **1–2** (1833), the Duchartre *Revue botanique*, **1–2** (1845–47), the Henfrey *Botanical Gazette*, **1–3** (1849–51), and the Miquel *Journal de botanique néerlandaise*, **1** (1861). In none of these were the included articles limited to the writings of the editors, but the pages were apparently open to any responsible contributor. Junk [17] gives some interesting data on the beginnings of botanical periodical literature.

Institutional versus Private Periodicals

There are, of course, some serials optimistically initiated by institutions and organizations, that, like most of the personally operated ones, languish and die after a relatively short period of issue. The causes for their demise are various, but not infrequently their disappearance may be explained by the sudden departure or death of the individual who initiated the enterprise. Thus the *Pomona College Journal of Economic Botany and Subtropical Horticulture* came to an untimely end when its founder, Charles F. Baker, left to accept an appointment in the Philippines. Its life was about three years, 1 (1911) to 3 (1913), actually forming one continuous volume of 482 pages. There is no very sharp line of distinction between what I have called personal serials and institution- or organization-sponsored ones. Occasionally a personally operated serial may be taken over as an institutional one, as illustrated by the Kellermann *Journal of Mycology*, which suspended publication in 1908 and was immediately taken over by the New York Botanical Garden under the title *Mycologia* in 1909, and still later, in 1933, was made the official organ of the Mycological Society of America, thus now enjoying both insti-

[17] Junk, W. Die Anfänge der Botanischen Zeitschriften-Litteratur. *Rara Hist.-Nat. Math.*, **1**: 37–39, 1903.

tutional and organizational support. Attention is also called to the fact that one of our leading botanical periodicals, *The Botanical Gazette,* was commenced by John M. Coulter in 1875 (the title of its small first volume being the *Botanical Bulletin*) as a privately published serial, and continued by Coulter and his associates until Vol. **21** (1896) as such, when it became officially sponsored by the University of Chicago.

The chances of survival is infinitely better with institution- or organization-sponsored periodicals than with those hopefully initiated by individuals. Of over thirty personal botanical serials commenced between 1875 and 1941 in the United States, only a very few continue to appear, and several of these few lead a distinctly tenuous existence; I say "tenuous," for volume one of the Gleason-Moldenke *Phytologia,* initiated in 1933, closed with number fifteen in 1941, an eight-year interval, and in 1941–42 only two small numbers of volume two have appeared, while the Eastwood-Howell *Leaflets of Western Botany,* established in 1932, is now in its third volume. The Jepson *Erythrea* is in a category by itself. After seven volumes were issued, its publication was suspended in 1899. Twenty-two years later volume eight, numbers 1–12, suddenly appeared, and after another lapse of sixteen years volume eight, number 13, was issued in December, 1938; there is no evidence that this serial is defunct. The Brandegee-Eastwood-Brandegee *Zoe* also suffered a lapse. Volumes one and two (1890–1892) were issued by T. S. Brandegee, volume three (1892–93) by Alice Eastwood, volume four (1893–94) by a group of six individuals; it was then suspended. Six years later it was resurrected by Katherine Brandegee, eleven numbers, forming the incomplete volume five, appearing between 1900 and 1908. With records like these is it any wonder that librarians grow gray? I think it is evident that many of those who initiated these "private" periodicals were individualists, impatient of delay in publication and firmly convinced that what they had to offer was worthy of publication. They did not hesitate to back their convictions by publishing their serials at least partly at their own expense, for rarely have these children of the botanical mind been wholly supported by receipts from sales and subscriptions. In botany, as in other fields of natural science, the demand for technical, semitechnical, and sometimes even popular serials is limited, and those that have succeeded in maintaining themselves over a long term of years have been in general those that have been subsidized by institutional funds or provided for through membership dues in special societies. The editorial and other work associated with these privately operated publications has always been a labor of love, for the profit incentive is absent.

Opinions of Rafinesque's Work

Doubtless there are those who would most gladly outlaw all or most of Rafinesque's systematic papers were it possible to do so, but, with others, I hold that the good should be retained, in spite of the often rather unorthodox methods of presentation and publication. As expressed by Underwood:[18]

> He published voluminously and so miscellaneously that some of his papers are still coming to light. Much of his work is worthless, yet there are veins of good interlarded among the bad that it still remains the task of the future to sift and save. In his crazy notions regarding the multiplicity of species, Rafinesque has no equals, a few weakling imitators, and only one real successor.[19]

No matter what we, our predecessors, and our successors may think of Rafinesque and his work, we cannot afford to forget Dr. John Torrey's statement[20] regarding his ability *as a naturalist,* for he, knowing Rafinesque personally, writing in about 1821, states: "He is the best *naturalist* I am acquainted with, but he is too fond of novelty. He finds too many new things. All is new! new!!" On which Mr. James remarks: "A fault which is common to many even of our modern scientists, and for which Rafinesque ought not to be blamed any more than another."

It is, of course, regrettable that Rafinesque did not conform more closely to the concepts of his contemporaries, in reference to the delimitation of genera and of species, and particularly in his methods of presentation. That he was a very

[18] Underwood, L. M. *Pop. Sci. Monthly,* **70**: 511–512, 1907.

[19] By "only one real successor" Dr. E. L. Greene is indicated, and I would certainly add the names of Michael Gandoger (1850–1926) and Augustin Abel Hector Léveillé (1863–1918). I am not so sure that some of our more recent American botanists do not qualify. I should add here that Underwood did not originate this characterization of Greene; see Katherine Brandegee (*Zoe,* **4**: 420, 1894), who, in reviewing a paper by Greene, states: "A year or two before his death Dr. Gray dubbed the author 'The new Rafinesque.' In this he was unjust to Rafinesque who was at once a great egotist, a little mad, and somewhat of a genius. Prof. Greene lacks the genius."

[20] James, J. T. A letter from Dr. Torrey to Amos Eaton. *Bot. Gaz.,* **8**: 289–291, 1883.

keen observer and a naturalist of most unusual attainments is clear, but where he failed was in his concepts of the limits of genera and species and in his methods of presentation of the published word.

His was the opportunity, as a pioneer botanist, working on the then relatively little-known North American flora, to clarify rather than to confuse, and yet all who work with his published data will agree that he confused rather than clarified. Manifestly his name was anathema to his contemporaries, and as he left literally hundreds of problems for later generations of systematists to solve, I am afraid that to most of my contemporaries his name is still anathema. No more scathing criticism can be levelled at a modern taxonomist than to characterize his work as Rafinesquian. And yet such a conservative botanist as Asa Gray, writing about Rafinesque's work in 1841, states:[21]

> It is indeed a subject of regret, that the courtesy which prevails among the botanists of the present day, (who are careful to adopt the names proposed by those who even suggest a new genus) was not more usual with us some twenty years ago. Many of Rafinesque's names should have been adopted; some as a matter of courtesy, and others in accordance with strict rule.

Modern Investigations of the Work of Early Botanists

I know that I have been subjected to published and spoken criticism by various botanists working on problems involving species described by Rumphius, Blanco, Llanos, Burman, Osbeck, and Loureiro, particularly when my conclusions have shown the necessity, under the prevailing rule of priority, of displacing a widely used binomial in favor of an earlier one, in accordance with the principle of priority. On the publication of my study of Loureiro's genera and species,[22] a former associate who knows little or nothing about the problems involved, is reported to have expressed the opinion that it ought to be suppressed! Why not go the whole distance and suppress Loureiro's original work as well as the publications of all botanists whose species are not actually represented by extant types, including even the high percentage of the Linnaean species that fall in this category? We should at least be logical. In some cases pages of print have been used to show, in great detail, how my conclusions cannot possibly hold; and in general the rebuttal argument *contra* is no more convincing than the original argument *pro*. In any case of disagreement the position of the dissenter could have been stated in a very few words rather than perpetrating pages of mere verbiage regarding, after all, what are really very minor and really inconsequential points.[23] One almost suspects that the objective of these critics might be to discredit all work on several thousand species on the basis of differences of opinion as to the status of a very few only!

In reference to Rafinesque's taxonomic botanical work I feel confident that with a proper study of his descriptions, associated with special field work and a study of reference material collected in the regions that he explored, many of his entities, the names of which still encumber botanical literature, can definitely be placed, this even when his type specimen is no longer extant; in other words, by an application of the principles developed in reference to the very numerous problems associated with species proposed by early oriental authors, a reasonably high percentage of Rafinesque's species can be placed even in the absence of type specimens. I am convinced that in the United States there is today, in relation to Rafinesque, as nice a series of problems to be solved, as I encountered in my attempts to elucidate the species based by Linnaeus and his successors on Rumphius' *Herbarium Amboinense* (1741–1755), some 350 new binomials in all being involved; those described by Blanco in his *Flora de Filipinas* (1837, 1845); Llanos in his *Fragmentos de algunas plantas de Filipinas* (1851); and Loureiro in his *Flora Cochinchinensis*[24] (1790). Here, supplemented by somewhat similar studies based on entities described by Osbeck (1757) and by Burman (1768), between 5500 and 6000 descriptions are involved

[21] Gray, A. *Am. Jour. Sci.*, **40**: 234, 1841.

[22] Merrill, E. D. A commentary on Loureiro's "Flora Cochinchinensis." *Trans. Am. Philos. Soc.*, n.s., **24**(2): 1–445, 1935.

[23] See particularly Furtado, C. X. Palmae Malesicae II, *Gard. Bull. Straits Settlements*, **8**: 159–163; IV, *op. cit.*, 321–338, 1935; The typification of *Rhus javanica* L., *op. cit.*, **10**: 330–335, 1939; and with more restraint, Corner, E. J. H. Notes on the systematy and distribution of Malayan phanerogams I, *op. cit.*, **10**: 1–55; II, 56–81; III, 239–329, 1939.

[24] Merrill, E. D. An interpretation of Rumphius's Herbarium Amboinense. *Philippine Bur. Sci. Publ.*, **9**: 1–595, 2 maps, 1917.

——. Species Blancoanae: A critical revision of the Philippine species of plants described by Blanco and by Llanos. *Philippine Bur. Sci. Publ.*, **12**: 1–423, 1 map, 1918.

——. A commentary on Loureiro's "Flora Cochinchinensis." *Trans. Am. Philos. Soc.*, n.s., **24**(2): 1–445, 1935.

appertaining to various genera, species, varieties, and forms, very many of which were proposed as new, with, for the most part, no type specimens extant. Should we abandon the numerous new names proposed merely because no type specimens were preserved, and thus admit our inability to interpret a species from the description, supplemented by a study of the material from the type localities, together with habitats, local names, time of flowering, economic uses, and such other data as are given in the original descriptions? To admit inability to interpret such descriptions places us in the egotistical category of assuming that we can describe species that others can recognize; but I wonder just how many of our descriptions would be any more intelligible to other workers than are those of Blanco, Loureiro, and numerous other early authors, if we did not preserve our type specimens that may be inspected in case of any doubt as to the status of a proposed genus or species?

In the case of all binomials based on the descriptions and illustrations in Rumphius, all species described by Blanco and most of those described by Llanos, there are no extant specimens or types to represent them. In the case of Loureiro there are extant specimens representing only about 300 of the 1292 species that he described. Thus, for the most part, the interpretations of genera and of species proposed by these authors must be based primarily on the published record. When, however, this published record is investigated in connection with field work prosecuted in the classical localities, together with a study of ample botanical collections from these *loci classici,* it becomes possible definitely to place a high percentage of the binomials that have vexed taxonomists from the time that they were proposed.

The Challenge of Rafinesque's Work

The same challenge exists in the United States in reference to much of Rafinesque's work, and has existed for over a hundred years. True, many of Rafinesque's genera and species have been accepted by all botanists and their characters and relationships are thoroughly understood. While some of his very numerous species are represented by extant, authentically named or type specimens, most of his actual herbarium specimens, on which he based his descriptions, were discarded by Elias Durand as valueless. Pennell [25] tells the sad story of what happened after Durand acquired the Rafinesque herbarium, quoting Durand's own words: "I did not care much for his own [Rafinesque's] plants. I knew that his specimens were miserable, but he had come to the possession of Mr. Collins' herbarium . . . Mr. Collins' collections were to me a most valuable acquisition." Pennell himself states: [26]

> Evidently no care was given to conserving the types of Rafinesque's many species, but Durand's effort was to salvage what he could of Collins' precious herbarium. Rafinesque's later works received only scorn from his contemporaries, and it was then supposed that his proposed new species could be simply ignored, but subsequently, with the coming of precise and impersonal rules of nomenclature, it is realized that every scrap should have been saved that could help interpret his work.

What a thorough task Durand did in eliminating the Rafinesque specimens, the actual types of very numerous species, is indicated by Dr. Pennell's and Mrs. Chase's statements, that in the Herbier Durand at Paris the former could find specimens of only eight of about sixty species of Scrophulariaceae that Rafinesque described, while the latter reports that not a single specimen was preserved representing the various genera and species of Gramineae that Rafinesque named and characterized.

Opposed to the relatively small percentage of Rafinesque's genera and species that are universally accepted, there is a very high percentage that still remain, not being understood, in that limbo of *species ignotae* or *species incertae;* and doubtless many of these have been renamed and redescribed by the generations of botanists that have succeeded Rafinesque.

Besides describing hundreds of new genera and several thousand new species on the basis of actual specimens (and most of these specimens, as noted above, were later thrown away by Durand), Rafinesque also coined a very large number of new generic and specific names based on the publications of other authors and appertaining to the floras of Asia, Europe, Africa, Malaysia, and Australia, as well as those proposed by him in substitution for names of North American species used in the works of Pursh, Torrey, Eaton, Nuttall, Barton, Elliott, Muhlenberg, de Candolle, Bigelow, Michaux, and others. Thus, in study-

[25] Pennell, F. W. Botanical collectors of the Philadelphia local area. *Bartonia,* **21**: 38–57, 1942 (pp. 50–54).
[26] Pennell, F. W. *Bartonia,* **21**: 52, 1942.

ing all of Rafinesque's new generic and specific names, a great deal more than a mere study of North American species is involved. Any botanist who has the temerity to reinstate a Rafinesque genus or binomial, reducing some currently used name proposed by a later author to synonymy, may expect short shrift from his conservative contemporaries in spite of the fact that in nomenclature we are supposed to work under the general rule of priority. It is admitted that some botanists have made serious attempts to evaluate and interpret many of Rafinesque's genera and species, but the fact remains that no individual has, as yet, accepted the challenge to examine the Rafinesquian nomenclatural problems *in toto*. Until this is done, many of his names will remain in that most unsatisfactory category of species *ignotae vel incertae*. When the work is done, as some day it must be, undoubtedly many of Rafinesque's names will replace those proposed by his successors and now generally accepted, unless ways and means can be devised to extend the list of *nomina generica conservanda* and to extend this principle to a list of *nomina specifica conservanda;* and so far the International Botanical Congresses have consistently refused to entertain the latter idea.

Field Knowledge in Reference to Rafinesque Problems

Even when it is clear from a Rafinesque description that he placed a species in the wrong group, if one really knows the flora of a region, it is often possible to place such a species through the process of elimination. *Evolvulus? cuneifolius* Raf. (*Fl. Tellur.*, **4**: 82, 1838) was based on a fruiting specimen collected by Rafinesque in the pine barrens of New Jersey, a region having a limited and a characteristic flora. The species has never been placed, and in 1934 Van Oostroom [27] left it among the doubtful and little known species of *Evolvulus*. I then asked Dr. H. N. Moldenke, who is very familiar with the New Jersey pine-barren flora, to scan Rafinesque's description and compare it with those species in various groups of plants that he knew as constituent elements of this limited flora. He reports that *Evolvulus? cuneifolius* Raf. is unquestionably a synonym of *Stylisma Pickeringii* (M. A. Curtis) A. Gray, and thus the species becomes an excluded one under *Evolvulus*, and *Plesilia* Raf. (*New Fl. N. Am.*, **4**: 56, 1838), here most casually published as a new genus: "probably a subgenus or G. *Plesilia* Raf. but the corolla and stamens must be described, the calix as in *Stylisma*," must be eliminated from the synonymy of *Evolvulus* and transferred to the synonymy of *Stylisma* Raf. (1825). Van Oostroom (*op. cit.*, 19) left *Plesilia* Raf. as a doubtful synonym of *Evolvulus*. This is the process of elimination, but to apply it successfully one must have an intensive knowledge of the flora of the region involved, for a monographer of a natural group could scarcely be expected to place a species so sketchily described as *Evolvulus? cuneifolius* Raf. = *Plesilia cuneifolius* Raf. from the published record alone, when its original author placed it in the wrong genus.

The task of interpreting Rafinesque's genera and species on the basis of his original, usually very short descriptions, supplemented by field work in the classical localities, will be no sinecure. Fernald has indicated some of the difficulties involved.[28] He states that this most erratic student has made unending difficulties for American, and although they apparently do not realize it, European botanists as well; I would amplify this by stating that in his later works Rafinesque originated difficulties not only for the student of the floras of North America and Europe, but also for those concerned with the floras of South America, Africa, Asia, Malaysia, and Australia. While correctly stating that much of Rafinesque's work is too obscure for clarification, he calls attention to the fact that some of his books—for instance the extensive *Autikon Botanikon* (1840) (this is true also of the *Flora Telluriana*, 1837–38, and the *Sylva Telluriana*, 1838)—contain accurate descriptions of genera and species which it is a duty to maintain. He further states:

> The task of sifting the comparatively few perfectly sound grains from the chaff and the distorted or unrecognizable grains is a thankless one and, above all, it should be undertaken only by those with intimate knowledge of the floras concerned. . . . I should have, consequently, the gravest misgivings if assigned the unwelcome task of interpreting much of Rafinesque's publication.

The Content of Rafinesque's Overlooked Paper of 1834

Regarding de Candolle's work, Rafinesque (*Act. Soc. Linn. Bordeaux*, **6**: 261, 1834) says:

[27] Van Oostroom, S. J. A monograph of the genus Evolvulus. *Med. Bot. Mus. Herb. Rijksuniv. Utrecht*, **14**: 1–267, 1934 (p. 244).

[28] Fernald, M. L. Some genera and species of Rafinesque. *Rhodora*, **34**: 21, 1932.

"De tous les ouvrages généraux sur la Botanique, celui de De Candolle, étant le plus récent, le meilleur et le plus accrédité, réclame l'attention de tous les botanists pour le perfectionner." While one may agree with few, perhaps none, of Rafinesque's proposed changes in nomenclature, one may sympathize with him in that his corrections and emendations to de Candolle's classical work remained in the limbo of overlooked contributions from the time that his paper was published late in 1834 until the present, somewhat over a century; and there is some evidence that Rafinesque himself forgot all about this short paper. The thirty-one generic names proposed as new, as well as fifteen binomials, other than a few later accepted by Rafinesque and published *de novo* elsewhere, have never been cited by succeeding botanists, perhaps chiefly for the reason that this paper was overlooked by Steudel and by the compilers of *Index Kewensis*.

In view of the fact that de Candolle applied the general rule of priority, Rafinesque, with some justification, calls attention to the fact that *Odostemon* Raf. 1817, is not even cited as a synonym of *Mahonia* Nutt. 1818; that *Stanleya* Nutt. 1818, is the same as *Podolobus* Raf. 1817; that *Idistaria* Nutt. 1818 has two earlier names, *Kranushia* Raf. 1808 and *Thyrsanthus* Elliott 1817; why adopt *Ptilophylum* Nutt. 1818 in place of *Purshia* Raf. 1808, and then apply the generic name *Purshia* to another group?; that *Discopleura* DC. 1830 is the same as *Ptilimnium* Raf. 1825; that *Archemora* DC. 1830 is Rafinesque's own genus *Oxipolis* (1824); and regarding *Cryptotenia* (*Cryptotaenia*), "Pourquoi substituer *Cryptotenia*, mauvais nom formé de *Tenia* un genre de vers, pour mon *Cyrtospermum*, 1817, *qu'il cite?*" "Pourquoi continuer le nom d'-*Helianthemum* identique de *Helianthus!* je l'ai changé en *Anthelis* 1814, *Chloris Etnensis.*" "Les genres *Hamiltonia* et *Pyrularia* sont absolutament les mêmes et fondés sur la même espèce, pourquoi donc les separer, tandis que le genre *Spermadyctium* est réuni à *Hamiltonia!*" Of course the answer to most of these criticisms is Rafinesque's unorthodox and often utterly inadequate methods of publication.

On pages 268–269 he notes a curious geographic error under *Misodendron*—" il est appelé *boréal* parce qu'on l'a cru natif de Staten-Island près de New York, tandis qu'il faut lire *australe* et natif de *Staten-Land* ou *l'Isle des États*, près du cap Horn." As to certain admitted species, he states that *Pulsatilla nuttalliana* Spr. is his *P. cyanea*, described in 1817 in his *Florula Mandanensis*, having been discovered by Bradbury and not by Nuttall; that *Drosera intermedia*, var. *americana* DC. is the same as *D. foliosa* Elliott; that the American species of *Virgilia* are very different from the African ones and form his genus *Cladrastis*, with various other notes of this nature.

Below is given a summary of the novelties proposed in Rafinesque's paper under discussion. In each case I have quoted all that he wrote regarding each individual entity.

Under *Ranunculus* (DC. *Prodr.*, **1**: 32, 1824): "Le *R*[*anunculus*] *pusillus* à 3 pétales formera le genre *Sardonula* Raf." (*Act. Soc. Linn. Bordeaux*, **6**: 262, 1834).

Under *Anemone* (DC. *Prodr.*, **1**: 16, 1824): "Les espèces à Périgone 5-phyl. inégal, forment mon genre *Abalemis*. 1824. J'en cinq espèces types: les *Anemone Virg.* et *Pensylv.* des auteurs, et deux espèces nouvelles" (*Act. Soc. Linn. Bordeaux*, **6**: 262, 1834). *Abelemis petiolaris* Raf. "in herb. Paris" was published by Britton (*Ann. N. Y. Acad. Sci.*, **6**: 223, 1892) as a synonym of *Anemone virginiana* Linn. I have not been able to locate any publication of *Abalemis* (or *Abelemis*) by Rafinesque in 1824, and this may refer only to a manuscript, or again, it may refer to some as yet undetected published paper by Rafinesque. I take it that *Abalemis* is the preferred form as this is the one that Rafinesque actually published, although *Abelemis* may be correctly formed.

Under *Macrotys* (DC. *Prodr.*, **1**: 64, 1824): "DC. à changé mon *Macrotrys* abrégé de *Macrototrys* en *Macrotis* qui signifie grande oreille! c'est un erreur" (*Act. Soc. Linn. Bordeaux*, **6**: 262, 1834). But Rafinesque was in slight error here, for de Candolle used the form *Macrotys*, not *Macrotis*. It was published by Rafinesque (*Jour. Bot.*, **2**: 170, 1809) as *Macrotrys*.

Under *Hesperis* (DC. *Prodr.*, **1**: 188, 1824): "D'après l'avis de DC., j'ai étudié de nouveau les espèces et variétés de *Hesperis pinnatifida* et en ai formé le genre *Oclorosis* dont j'ai trois espèces, etc." (*Act. Soc. Linn. Bordeaux*, **6**: 263, 1834). De Candolle admitted the species as *Hesperis? pinnatifida* Michx. It is *Thelypodium pinnatifidus* S. Wats. = *Iodanthus pinnatifidus* (Michx.) Steud. The generic name *Oclorosis* Raf. is four years older than *Iodanthus* Torr. and Gray (1838), but it was not validly published, hence no change in generic nomenclature is indicated.

Under *Drosera* (DC. *Prodr.*, **1**: 317, 1824): "Notre *Dr*[*osera*] a feuilles rondes, n'est ni le

rotundifolia, ni *l'orbiculata* d'Europe; mais mon *Dr. spatulosa*" (*Act. Soc. Linn. Bordeaux,* **6**: 263, 1834).

Under *Polygala* (DC. *Prodr.,* **1**: 321–333, 1824) several genera are proposed as follows:

"*Asemeia,* Raf. étam. 8, monadelphe, fleurs racémeuses, dont deux espèces: *A. rosea* et *A. carnea* (*P. pubescens* des auteurs) *A. carnea* est glabre" (*Act. Soc. Linn. Bordeaux,* **6**: 264, 1834). This is an earlier publication of the generic and both specific names than that currently given in botanical literature: Rafinesque, *New Fl. N. Am.,* **4**: 88 (1836) 1838.

"*Iridisperma* R. 1814. *P*[*olygala*] *paucifolia,* fleurs axill." (*Act. Soc. Linn Bordeaux,* **6**: 264, 1834).

"*Senegaria,* R. fleurs en épis, feuilles alt. cor. glanduleuse" (*Act. Soc. Linn. Bordeaux,* **6**: 264, 1834).

"*Leptrochia,* R. 6 étamines, cor. ent. feuilles verticales, fleurs en épis" (*Act. Soc. Linn. Bordeaux,* **6**: 264, 1834).

"*Pylostachya,* R. étamines 4, cor. frang. fleurs capitulées (*P. lutea,* etc.)" (*Act. Soc. Linn. Bordeaux,* **6**: 264, 1834).

"*Corymbula* R. les espèces corymbeuses" (*Act. Soc. Linn. Bordeaux,* **6**: 264, 1834). This replaces Rafinesque's later publication in *New Fl. N. Am.,* **4**: 88 (1836) 1838.

"*Anthallogea* R. Étamines 7–8 stigm. bilabié, cor. 3-lob. sombre (*P. polygama*)" (*Act. Soc. Linn. Bordeaux,* **6**: 264, 1834). This was published four years later by Rafinesque (*New Fl. N. Am.,* **4**: 88 (1836) 1838) as *Anthalogea,* but here also without transferring the specific name as indicated in *Index Kewensis;* this transfer was validated by Nieuwland (*Am. Midl. Nat.,* **3**: 180, 1914) as *Anthalogea polygama* (Walt.) Nieuwland, although I should interpret the *Index Kewensis* reference as a valid transfer and credit the binomial to Jackson as *Anthalogea polygama* Jacks.

Under *Silene* (DC. *Prodr.,* **1**: 367–385, 1824): "Ce genre nombreux demande une réforme; les espèces jadis parmi les *Cucubales,* à pétales laciniés non appendicés, forment mon genre *Ixocaulon*" (*Act. Soc. Linn. Bordeaux,* **6**: 264, 1834).

Under *Modiola* (DC. *Prodr.,* **1**: 435, 1824), as a section of *Malva:* "Il y à un genre de coquille de ce nom: il faudra donc changer en *Diadesma* R. ce genre botanique" (*Act. Soc. Linn. Bordeaux,* **6**: 264, 1834). This publication of *Diadesma* is two years earlier than the current reference to Rafinesque's *New Fl. N. Am.* (**1**: 41, 1836).

Under *Hypericum* (DC. *Prodr.,* **1**: 543–555, 1824): "Le genre *Saiothra* doit être restitué pour les espèces à capsule uniloculaire" (*Act. Soc. Linn. Bordeaux,* **6**: 264, 1834). *Saiothra* is a typographical error for *Sarothra* Linnaeus, a genus that de Candolle does not mention.

Under *Thermopsis* (DC. *Prodr.,* **2**: 99, 1825): "C'est mon genre *Scolobus* ou *Verzinum* de 1817" (*Act. Soc. Linn. Bordeaux,* **6**: 265, 1834).

Under *Prunus* (DC. *Prodr.,* **2**: 532–534, 1825): "J'en ai déscrit 4 espèces nouvelles, *Pr. sessilis* [err. *sessislis*], *Pr. aurantiaca,* etc., et j'en prépare une monographie avec les cerisiers de l'Amérique septentrionale" (*Act. Soc. Linn. Bordeaux,* **6**: 265, 1834).

Under *Cerasus* (DC. *Prodr.,* **2**: 535–541, 1825) Rafinesque recognized the genera *Cerasus* and *Padus,* and the new: "*Orospodias.* Calice campanule 5-fid., fl. en corymbes" (*Act. Soc. Linn. Bordeaux,* **6**: 265, 1834).

Under *Spiraea* (DC. *Prodr.,* **2**: 541–546, 1825) as *Spirea:* "Ce genre réclame une reformé complète; il faut restituer les genres des anciens botanistes; auxquels DC. en a ajouté deux: *Phylocarpos* et *Chamedrys:* et j'en ai aussi ajouté deux autres" (*Act. Soc. Linn. Bordeaux,* **6**: 265, 1834). De Candolle (*Prodr.,* **2**: 542, 1825) adopted as sections *Physocarpos* Camb. (not *Phylocarpos*) and *Chamaedryon* Ser. (not *Chamedrys*). Rafinesque's *one* new genus is:

"*Thecanisia* R. [p. 265] Cal. pers. refl. st. *paucis.* Pistil 5-stipit. stylosa, stigma cap. capsis theca 5 inaequ. 1–3 Sp. stipitatis. [p. 266] Fl. panic., type *Sp. lobata.* Mais j'en ai cinq espèces en herbier. *Th. lobata, angustata, angulata, amplifoliata* et *discolor.* Il y en a d'autres petits en Sibérie et Orgon." (*Act. Soc. Linn. Bordeaux,* **6**: 265–266, 1834). This publication of *Thecanisia* is three years earlier than that listed in *Index Kewensis:* Raf. *New Fl. N. Am.,* **2**: 38 (1836) 1837. Rafinesque apparently forgot this earlier publication of the genus, and in 1837 he described it again with four species, *T. angustifolia* Raf., *T. discolor* Raf., *T. lobata* Raf., *T. purpurea,* and later (*Sylva Tellur.,* 152, 1838) added two others, *T. palmata* Raf. and *T. Ulmaria* Raf. *Thecanisia angustata* Raf. is a *nomen nudum* and may be assumed to be the same as *T. angustifolia* Raf., while two binomials, *T. discolor* and *T. lobata,* are three years earlier than their appearance in the *New Fl. N. Am.* (**2**: 38 (1836) 1837), but *T. discolor* Raf. only as a *nomen nudum. T. lobata* Raf. (1834) is validly published, being based on *Spiraea lobata* Jacq. A description of

Epicostorus (err. *Espicostorus*), with a single species *E. montana,* follows, but this was originally described by Rafinesque in the *Atlantic Journal* (**1**: 144, 1832). The curious entry on page 266 following *E. montanus* Raf., *"Spica monogyna.* Teney d'Orgon," is an error for *"Spiraea monogyna* Torrey d'Oregon."

Under *Amelanchier* (DC. *Prodr.,* **2**: 632, 1825): "Pourquoi admettre en botanique le nom français *d'Amelanchier* au lieu *d'Axonia* Persoon? encore si l'on l'avait latinisé! en *Amelanchus!* mais cette erreur si palpable est cedependant admise!" (*Act. Soc. Linn. Bordeaux,* **6**: 266, 1834). This replaces the similar action eighty years later by Vollmann, who (*Fl. Bayern,* 453, 1914) accepts the name as *Amelancus* Med., explaining in a footnote: "Ich nehme daher den Vorschlag von Dr. Franz Müller (Graz 1913) an und schreibe *Amelancus.*"

Under *Ammannia* (DC. *Prodr.,* **3**: 77–80, 1828: "Les sous genres méritent de former les genres *Cesdelia* DC. et *Boykiana* Raf." (*Act. Soc. Linn. Bordeaux,* **6**: 266, 1834). De Candolle recognized four sections of *Ammania—Apetalae, Tetrapetalae, Pentandrae,* and *Diplostemoneae.* I have been unable to locate any publication of a *"Cesdelia* DC.," and *Boykiana* is manifestly a typographical error for *Boykinia* Raf. (*Aut. Bot.,* 9, 1840). This abortive "publication" of *Boykinia* Raf. does not, I believe, invalidate *Boykinia* Nutt. (1834), and Rafinesque's second consideration of his *Boykinia* did not appear until four years after Nuttall's genus had been characterized and published.

Under *Adenarium* (DC. *Prodr.,* **3**: 366, 1828): "Les genres *Adenarium* R. 1817, et *Adenaria* Kunth 1821, ne peuvent pas subsister tous deux étant synophones: le dernier doit recevoir le nom de *Decadonia*" (*Act. Soc. Linn. Bordeaux,* **6**: 267, 1834).

Under *Bartonia* (DC. *Prodr.,* **3**: 339, 1828): *"Bartonia sirus* [*sirus* err. for Sims] est postérieur à *Bartonia,* Wild. [correctly Muhlenberg], et doit se changer en *Nuttalia* R. 1817, ou bien *Rhoanthus* Raf." (*Act. Soc. Linn. Bordeaux,* **6**: 267, 1834).

Under *Claytonia* (DC. *Prodr.,* **3**: 360–362, 1828): *"Claytonia nemorosa* DC. Willd. paraît aussi devoir former un genre que je nomme *Ditomaga nemorosa*" (*Acta Soc. Linn. Bordeaux,* **6**: 267, 1834). This proposed new generic name is a synonym of *Irlbachia* Mart. (1827). The type of *Claytonia nemorosa* Willd. was a specimen collected by Humboldt in Colombia "in umbrosis prope Javitam Orinocensium." Its synonymy is:

Irlbachia nemorosa (Willd.) comb. nov.

Claytonia nemorosa Willd. ex Roem. & Schult, *Syst. Veg.,* **5**: 436, 1819; H.B.K. *Nov. Gen. Sp. Pl.* **6**: 64 (fol.) 80 (quart.), 1823 *in nota;* DC. *Prodr.,* **3**: 362, 1828.

Ditomaga nemorosa Raf., *Act. Soc. Linn. Bordeaux,* **6**: 267, 1834.

Irlbachia Bonplandiana Fenzel, *Nov. Stirp. Dec.* 12, 1839; Griseb. in DC. *Prodr.,* **9**: 72, 1845.

All binomials are based on a single collection.

Under *Seseli* (DC. *Prodr.,* **4**: 144, 1830): "DC. n'a pas de genre *Marathrum,* mais *Hyppomarathrum,* où est mon *Marathrum* de 1819, que j'ai changé en *Adorium* [*Neogen.,* 3, 1825] pour éviter la coincidence?" (*Act. Soc. Linn. Bordeaux,* **6**: 268, 1834). *Hyppomarathrum* is a variant spelling of *Hippomarathrum,* and de Candolle uses it as a sectional name, not as a genus, under *Seseli.*

Under *Leptocaulis* (DC. *Prodr.,* **4**: 107, 1830): *"Leptocaulis divaricatus* DC. qui a été balloté dans les genres *Ammi, Daucus, Sison* et *Ligusticum,* est le type de mon genre *Lepisperma,* qui doit différer des autres *Leptocaulis*" (*Act. Soc. Linn. Bordeaux,* **6**: 268, 1834).

Under *Panax quinquefolium* DC. (*Prodr.,* **4**: 252, 1830) Rafinesque states that there are several species in Asia and America: "J'en ai trois en herbier: *P. 5-fol. obovatum, cuneatum* et *lanceolatum!*" (*Act. Soc. Linn. Bordeaux,* **6**: 268, 1834). Of these Rafinesque later described only *Panax lanceolatum* Raf. (*New. Fl. N. Am.,* **4**: 57, 1838), and (*op. cit.,* 58) recognized *Panax americanum* Raf. var. *obovatum* Raf. *P. obovatum* Raf. and *P. cuneatum* Raf. are *nomina nuda,* as is *P. lanceolatum* Raf. of 1834.

Under *Oldenlandia* and *Hedyotis* (DC. *Prodr.,* **4**: 419–429, 1830): *"Oldenlandia* ou *Hedyotis uniflora* paraît non seulement un nouveau genre que je nomme *Edrastima uniflora,* mais ce n'est pas même une Rubiacée puisque les étamines sont opposées à la corolle. Ce sera donc un nouveau genre de la famille des Samolides avec *Samolus*" (*Act. Soc. Linn. Bordeaux,* **6**: 269, 1834). This comment refers to *Hedyotis glomerata* Ell. as interpreted in DC. *Prodr.,* **4**: 421, 1830, not to *Hedyotis uniflora* Ruiz and Pav. (DC. *loc. cit.*). *Oldenlandia uniflora* Linn. is the species of the eastern United States that Rafinesque had in mind, not the South American species, and it was cited by de Candolle as a synonym of *H. glomerata* Ell. It is the species currently recognized in all our manuals as *Oldenlandia uniflora* Linn. However, if the genus *Oldenlandia* Linn. be combined with

Hedyotis Linn., as many authors have done, then the proper generic name under the provisions of the International Code of Botanical Nomenclature is *Hedyotis,* as this was the name selected by those who first combined the two genera, Lamarck and J. E. Smith.

Under *Spermacoce* (DC. *Prodr.,* **4**: 552–561, 1830): "Les genres *Spermacoce* et *Diodia* sont si mal distingués que l'on ne peut pas les séparer, car ils n'ont aucun caractère exclusif selon DC., mais il y a de bons caractères dans les calices bidentés, 4-dentés égaux, 4-dentés inégaux, 5-dentés, 10-dentés, de plusieurs espèces, qui devront sans doute former autant de genres, comme suit:

Diodia, calice 2-denté.

Spermacon, calice 4-denté, égal.

Dioneiodon, R. cal. 4-denté inégal.

Decapenta, R. cal. 5–10-denté, peut être 2 genres aussi" (*Act. Soc. Linn. Bordeaux,* **6**: 269, 1834).

Of these four names, *Spermacon* Raf., *Dioneiodon* Raf., and *Decapenta* Raf. are new. Rafinesque forgot this use of *Decapenta,* and six years later (*Sylva Tellur.,* 165, 1838) used this same name for an entirely different lauraceous group, *Decapenta involucrata* Raf. (*loc. cit.*) = *Litsea sebifera* Pers. = *Litsea glutinosa* (Lour.) C. B. Rob.

Additions and Emendations to *Index Kewensis*

The following additions and corrections to *Index Kewensis* are indicated on the basis of a critical examination of this overlooked Rafinesque paper of 1834. Including minor variants, thirty-one generic names and fifteen binomials are involved. Of these, only six generic names and five binomials appear in *Index Kewensis,* but with references to places of publication later than 1834. May it be hoped that the next overlooked Rafinesque paper that is discovered (for there is no reason to believe that all of his publications have been located), will be less prolific in new generic and specific names than the paper that forms the subject of this contribution!

Abalemis Raf., *Act. Soc. Linn. Bordeaux,* **6**: 262, 1834 = *Anemone.*

Rafinesque indicates this as dating from 1824, but I have been unable to trace an earlier publication than that above given. "*Abelemis petiolaris* Raf." was published by Britton as a synonym of *Anemone virginiana* Linn. in 1892.

Amelanchus Raf., *Act. Soc. Linn. Bordeaux,* **6**: 265, 1834 = *Amelanchier.*

Anthallogea Raf., *Act. Soc. Linn. Bordeaux,* **6**: 264, 1834 = *Anthalogea* Raf. = *Polygala.*

Asemeia Raf., *Act. Soc. Linn. Bordeaux,* **6**: 264, 1834 = *Polygala.*

carnea Raf. (*loc. cit.*) = *P. pubescens.*

rosea Raf. (*loc. cit.*) = *P. pubescens.*

These replace the *Index Kewensis* entries for the genus and for both binomials, there given as Raf., *New Fl. N. Am.,* **4**: (1836) 1838.

Boykiana Raf., *Act. Soc. Linn. Bordeaux,* **6**: 266, 1834 = *Rotala.*

This is manifestly an error for *Boykinia* Raf.; see *Autikon Bot.,* 9, 1840. This genus is not the same as *Boykinia* Nutt. (1834), although *B. humilis* Raf. is entered in *Index Kewensis,* under Nuttall's genus. De Dalla Torre & Harms (*Gen. Siphon,* 340, 1903) and Koehne (*Pflanzenr.* **17** (IV, 216): 24, 1903) confuse the reference to *Boykinia* Raf. as "Ant. (sic!) Bot. (1817) 9"; the latter adding from *Index Kewensis,* "sec. C. Watson." Watson's entry is "Raf. Rev. 1817; Aut. Bot. 9."

He took the "Rev. 1817" directly from the *Autikon Botanikon,* which was published in 1840. The reference by Rafinesque to "Rev. 1817" may refer to an unpublished manuscript, or possibly to some as yet overlooked paper. I did not find it in the *American Monthly Magazine and Critical Review,* in which Rafinesque did publish various papers in 1817, and have failed to locate it elsewhere. A new entry should be made in *Index Kewensis* as follows:

Boykinia Raf., *Aut. Bot.,* 9, 1840 (*Act. Soc. Linn. Bordeaux,* **6**: 166, 1834, sphalm. *Boykiana, nomen*) = *Rotala* Linn. (Lythrac.).

humilis Raf., *Aut. Bot.,* 9, 1840 = *Rotala ramosior* (Linn.) Koehne.

Cesdelia "DC." ex Raf., *Act. Soc. Linn. Bordeaux,* **6**: 266, 1834 = *Ammannia.*

Chamedrys Raf., *Act. Soc. Linn. Bordeaux,* **6**: 265, 1834 (error or deliberate change for *Chamaedryon* Ser.) = *Spiraea.*

Corymbula Raf., *Act. Soc. Linn. Bordeaux,* **6**: 264, 1834 = *Polygala.*

This is four years earlier than the *Index Kewensis* entry: Raf., *New Fl. N. Am.,* **4**: 88 (1836) 1838.

Decadonia Raf., *Act. Soc. Linn. Bordeaux,* **6**: 267, 1834 = *Adenaria* Kunth.

Decapenta Raf., *Act. Soc. Linn. Bordeaux*, **6**: 269, 1834 (non *Decapenta* Raf., *Sylva Tellur.*, 165, 1838) = *Diodia?*

Diadesma Raf., *Act. Soc. Linn. Bordeaux*, **6**: 264, 1834 = *Modiola.*

This publication of Rafinesque antedates by two years the currently accepted one: *New Fl. N. Am.*, **1**: 41, 1836.

Dioneiodon Raf., *Act. Soc. Linn. Bordeaux*, **6**: 269, 1834 = *Diodia.*

Ditomaga Raf., *Act. Soc. Linn. Bordeaux*, **6**: 267, 1834 = *Irlbachia.*

nemorosa Raf. (*loc. cit.*) = *Irlbachia nemorosa* (Willd.) Merr. (*supra*, p. 88).

Drosera spatulosa Raf., *Act. Soc. Linn. Bordeaux*, **6**: 263, 1834, *nomen nudum*, probably = *Drosera rotundifolia* Linn.

Edrastima Raf., *Act. Soc. Linn. Bordeaux*, **6**: 269, 1834 = *Hedyotis.*

uniflora Raf. (*loc. cit.*) = *Hedyotis uniflora* (Linn.) Lam. (*Oldenlandia uniflora* Linn.).

Espicostorus Raf., *Act. Soc. Linn. Bordeaux*, **6**: 266, 1834, a typographical error for *Epicostorus* Raf. (*Atl. Jour.*, **1**: 144, 1832).

Hyppomarathrum Raf., *Act. Soc. Linn. Bordeaux*, **6**: 268, 1834 = *Hippomarathrum* Hall.

Iridisperma Raf., *Act. Soc. Linn. Bordeaux*, **6**: 264, 1834 = *Polygala.*

Rafinesque dates *Iridisperma* as 1814, but I have been unable to locate it in any of his numerous papers that are available. It may have been published in some overlooked paper, or it may have been merely a manuscript name.

Ixocaulon Raf., *Act. Soc. Linn. Bordeaux*, **6**: 264, 1834 = *Silene.*

Lepisperma Raf., *Act. Soc. Linn. Bordeaux*, **6**: 268, 1834 = *Spermolepis.*

Leptrochia Raf., *Act. Soc. Linn. Bordeaux*, **6**: 264, 1834 = *Polygala.*

Oclorosis Raf., *Act. Soc. Linn. Bordeaux*, **6**: 263, 1834 = *Iodanthus.*

Orospodias Raf., *Act. Soc. Linn. Bordeaux*, **6**: 265, 1834 = *Prunus.*

Panax cuneatum Raf., *Act. Soc. Linn. Bordeaux*, **6**: 268, 1834, *nomen nudum.*

lanceolatum Raf. (*loc. cit.*), *nomen nudum.* This is earlier than the *Index Kewensis* entry; *New Fl. N. Am.*, **4**: 57 (1836) 1838; but the description was not published until 1838.

obovatum Raf. (*loc. cit.*), *nomen nudum.*

In his *New Fl. N. Am.* (**4**: 58, 1838), Rafinesque considers this as *Panax americanum* Raf. var. *obovatum* Raf.

Phylocarpos Raf., *Act. Soc. Linn. Bordeaux*, **6**: 265, 1834, err. for *Physocarpos* Camb. = *Spiraea.*

This is given in *New Fl. N. Am.* (**3**: 73 (1836) 1838) as *Physocarpa* Raf.

Prunus aurantiaca Raf., *Act. Soc. Linn. Bordeaux*, **6**: 265, 1834, *nomen nudum.*

sessilis (err. *sessislis*) Raf., *Act. Soc. Linn. Bordeaux*, **6**: 265, 1834, *nomen nudum.*

Pylostachya Raf., *Act. Soc. Linn. Bordeaux*, **6**: 264, 1834 = *Polygala.*

Typified by *Polygala lutea* Linn., but this binomial was not transferred.

Rhoanthus Raf., *Act. Soc. Linn. Bordeaux*, **6**: 267, 1834 = *Bartonia* Muhlenberg.

Saiothra Raf., *Act. Soc. Linn. Bordeaux*, **6**: 264, 1834, typographical error for *Sarothra* Linn. = *Hypericum* Linn.

Sardonula Raf., *Act. Soc. Linn. Bordeaux*, **6**: 262, 1834 = *Ranunculus.*

Senegaria Raf., *Act. Soc. Linn. Bordeaux*, **6**: 264, 1834 = *Polygala.*

Spermacon Raf., *Act. Soc. Linn. Bordeaux*, **6**: 269, 1834 = *Spermacoce.*

Thecanisia Raf.,[29] *Act. Soc. Linn. Bordeaux*, **6**: 265, 1834 = *Spiraea.*

amplifoliata Raf., *Act. Soc. Linn. Bordeaux*, **6**: 266, 1834, *nomen nudum.*

angulata Raf. (*loc. cit.*), *nomen nudum.*

angustata Raf. (*loc. cit.*), *nomen nudum.*

discolor Raf. (*loc. cit.*), *nomen nudum.*

lobata Raf. (*loc. cit.*).

Verzinum Raf., *Act. Soc. Linn. Bordeaux*, **6**: 265, 1834 = *Cytisus.*

This replaces the *Index Kewensis* entry; *Sylva Tellur.*, 23, 1838. Rafinesque indicated this as dating from 1817, but I have found no other publication of it except the two references above given. Rafinesque's reference to 1817 may refer to an unpublished manuscript, possibly to an overlooked paper.

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[29] This reference is three years earlier than the *Index Kewensis* entry; *New Fl. N. Am.*, **2**: 38 (1836) 1837; and this applies also to the last two of the binomials here listed, under *Thecanisia.* The genus, with five species, appears again in Rafinesque's *Sylva Telluriana* (152, 1838).

MEDICINE AND THE AMERICAN PHILOSOPHICAL SOCIETY

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(*Read February 14, 1942, in Symposium on the Early History of Science and Learning in America*)

Physicians Who Were Members Before the Year 1768, in Which the First Class of Physicians Was Graduated from Our Oldest Medical School

Until the founding of the medical school of the College of Philadelphia, now the University of Pennsylvania, in 1765, there was no institution in the British Colonies at which an aspirant could study medicine and acquire a medical degree. Likewise, there were no legal restrictions on the practice of medicine and, therefore, anyone who so desired could call himself a doctor and practice medicine. The young man who wished to acquire a training in medicine used to apprentice himself to a physician and after serving him for some years would obtain a certificate from his preceptor which was deemed sufficient warranty for him to start out on his own. Scattered throughout the Colonies there were a number of physicians who had studied medicine in Great Britain or on the Continent before coming to this country. During the first half of the eighteenth century these foreign-trained physicians were the most important factors in medical education in the Colonies. Before 1750 a few young Americans who realized the need for a better training went abroad for further study before entering on the practice of their profession. After that date young men went over in increasing numbers and not only studied abroad but acquired a medical degree at one of the foreign medical schools.

Between the years 1743 and 1768, the year in which the College of Philadelphia conferred its first medical degrees, among the two hundred and fifty-one members of the American Philosophical Society fifty-one are listed as physicians, of whom forty-eight practiced in the British Colonies of North America or the West Indies, and three were foreigners. A brief review of their professional acquirements throws an interesting light on the status of the medical profession in the Colonial period.

Foreign Members of the Society Before 1770

A few of the foreign physicians who were members of the Society should be mentioned because of the influence they exerted on American medicine through the young students from this country who studied under them.

William Cullen (1712–1790), the first foreign physician, elected to membership in 1768, was instrumental in founding the medical school at Glasgow and was professor of medicine and chemistry for many years there and later at Edinburgh. The majority of American students who went abroad in such numbers after 1750 attended his courses at Edinburgh and his teaching influenced them very greatly.

Sir Charles Linné, M.D. (1707–1778), more generally known as "Linnaeus," the great Swedish botanist, taught in the University of Upsala. He originated the Linnaean classification of plants by their sexual organs and introduced the binominal nomenclature, giving to each plant a generic or group name and a specific or given name. In 1735 he published his great work *Systema Naturae,* in which he applied the binominal nomenclature to animals as well as plants. The most distinguished American student of Linnaeus was Adam Kuhn, of Philadelphia. Linnaeus maintained an active exchange of letters and specimens with many American members of the Society, among them Alexander Garden, of South Carolina, John Bartram, of Philadelphia, and Cadwallader Colden, of New York. He was elected a member of the Society in 1770.

Dr. John Fothergill (1712–1780), also elected in 1770, was a very rich and prominent Quaker physician of London. He made several important contributions to medical literature and had a very large practice. Being a dissenter he had gone to Leyden to get his medical degree. He had one of the finest botanical gardens in London.

Peter Collinson, a Quaker merchant of London, who acted as agent in England for the Library Company of Philadelphia, and was a correspondent of John Bartram, used his American connec-

tions to get seeds and plants from that country for himself and Fothergill. He had sent some electrical apparatus to the Library Company with which Franklin conducted his epoch-making experiments. When Collinson received Franklin's account of his experiments he showed it to Fothergill, who realized their importance and had them published in a pamphlet for which he wrote a preface (1751). When Franklin arrived in London, 1759, he and Fothergill became close friends and remained so ever after. All the young Americans who came to England to study medicine had letters to Franklin, and Fothergill helped him greatly in putting them on the right track in pursuing their studies, sending them to the Hunters in London, and later to Edinburgh to get their degrees. Through Franklin Fothergill became greatly interested in the Pennsylvania Hospital which Franklin had been very influential in founding in 1751. Besides subscribing liberally to the Hospital, in 1762, when Dr. William Shippen, Jr., was returning to Philadelphia, Fothergill sent over to the Hospital in his care some very beautiful anatomical casts and paintings which he thought would be useful in teaching. He also sent the Hospital a book which he hoped might form the nucleus of a medical library, as it did.[1]

Members of the Society in the Colonial Period Who Had Received Their Medical Education Before Coming to This Country

Seven of the forty-eight American physicians, who were elected members of the American Philosophical Society before 1768, had received their medical education before coming to America and all of them became eminent. Two of them, Dr. Thomas Graeme and Dr. John Kearsley, Senior, settled in Philadelphia. Unfortunately we do not know where they studied medicine. Dr. Thomas Graeme[2] (1688–1772), a Scotch physician, came over with Governor Keith in 1717. Although in 1751 he was appointed consulting physician to the Pennsylvania Hospital "to assist in Consultation in extraordinary cases" and his obituary in the Pennsylvania *Gazette*, September 9, 1772, says he was "for Half a Century at the Head of his Profession, as a Physician, in this City," I can find no information as to where he had received his medical education or whether he had a medical degree. Graeme may have had a large practice but it did not interfere with his participation in public affairs. He became a member of the Provincial Council and judge of the Supreme Court of the Province, and he was also Collector of the Port of Philadelphia.

John Kearsley[3] (1685–1772) came to Philadelphia in 1711. He is said to have been a surgeon in London but there is no record of his having obtained a degree in medicine. He acquired a very large practice in Philadelphia and had many pupils or apprentices, including Lloyd Zachary, John Redman and John Bard. He was an able architect, Christ Church in Philadelphia still remaining as a testimony to his skill. At his death he left a large sum of money to found Christ Church Hospital for the support of poor widows of the Episcopal Church, an institution which still carries on its good work.

A very distinguished member of the Society was the Englishman, John Mitchell[4] (1680–1768), of Urbanna on the Rappahannock, who came to Virginia from England about 1700. It has been impossible to ascertain where he received his medical degree. A. F. Pollard in his sketch of Mitchell's life in the *Dictionary of National Biography* states, "There were several John Mitchells at Oxford at the beginning of the eighteenth century, more than one at Cambridge, and one who entered Leyden on 12 Feb. 1712, but none of these can certainly be identified with the botanist." He was a well-known botanist, making several notable contributions to botanical literature,[5] and was honored by Linnaeus, who named the *Mitchella repens* for him. He was a very able physician with an extensive practice. He sent an account of the yellow fever, which had prevailed in Virginia from 1737 to 1742, to Benjamin Franklin. The latter gave it to Dr. Benjamin Rush and Rush attributed his success in the treatment of the disease to the information he derived from Mitchell's description of his experience with it.[6] Mitchell contributed several papers on scientific subjects to the Royal Society of London, of which he was elected a Fellow in 1748, in which year (or in 1747) he had returned to England to live. Mit-

[1] The casts and pictures are treasured at the Hospital in excellent condition. The book is Number One in the Library of the Hospital, the earliest medical library in this country.

[2] Elected 1743.

[3] Elected 1768.

[4] Elected to membership in 1743.

[5] *Dissertatio brevis de principiis botanicorum*, 1738, and *Nova plantarum genera*, 1741.

[6] Rush subsequently published Mitchell's manuscript in the *American Medical and Philosophical Register*, Vol. IV.

chell wrote several political treatises on the relations between Great Britain and the Colonies.[7] His greatest claim to fame probably rests on the map, which he published in 1755, of the British and French Dominions in North America, which was the chief source used in subsequent negotiations on the American boundaries of the two countries.[8]

Among the physicians who emigrated to the British Colonies we find a large proportion were Scotch. The reasons for this are sufficiently obvious. During the eighteenth century the University of Edinburgh was the leading medical school in Great Britain. The universities of Oxford and Cambridge not only were far behind in medical education but they were closed to those who would not subscribe to the Test Act. Edinburgh on the other hand had a remarkably brilliant medical faculty, including the two Monros, primus and secundus, William Cullen, Robert Whyt and Joseph Black, and as its students were not required to subscribe to any test, those who were not members of the Church of England went there in large numbers. Scottish medicine also exerted a great influence in London where some of the leading practitioners and teachers were trained in Scotland; William Smellie, the leading obstetrician, and James Douglas who ran him a close second, William and John Hunter and Cruikshank, the anatomist, are instances.

Among these Scotch emigrant physicians who were elected to the Society there are some who deserve special mention.

Dr. Cadwallader Colden (1688–1776), a native of Scotland, who was elected in 1743, got his M.D. at Edinburgh in 1708. Two years later he arrived in Philadelphia where he practiced medicine until 1715, when he went over to London. He remained there until 1718, when he recrossed the Atlantic and settled in New York. Soon after he abandoned medicine for public life, ultimately becoming lieutenant governor of the Province. Though Colden wrote several medical treatises he devoted most of his time to mathematics, botany and history. He published a *History of the Five Indian Nations,* and several essays on mathematics, physics and astronomy. Colden carried on an active correspondence with Benjamin Franklin, Fothergill, Collinson, Linnaeus and many other scientists. John Ellis, the English botanist, named the plant *Coldenia* in his honor.[9]

In the eighteenth century Charleston, South Carolina, was the home of many distinguished physicians and scientists. Dr. David Ramsay in his history of South Carolina, in 1808, states: "Before Dr. Rush began his brilliant career as an author, there were more experiments made, more observations recorded, and more medical writings ushered into public view by the physicians of Charleston, than of any other part of the American Continent."[10]

Lionel Chalmers[11] (1715–1777) is said to have graduated at Edinburgh and emigrated to this country soon after. He made meteorological observations throughout a number of years, and some of the results and their bearing on the climate and diseases of South Carolina were included in the *Transactions* of the American Philosophical Society for 1768.[12]

Alexander Garden[13] (1728–1791), who also practiced in Charleston, was born in Scotland, got his M.D. at Edinburgh in 1752. Garden went out to South Carolina in the same year. He was an able botanist and zoologist and a correspondent with Franklin, John Bartram, Cadwallader Colden and other scientists. He was elected a Fellow of the Royal Society of London in 1773. Garden introduced the *Spigelia marilandica,* or pink root, into medicine for use as a vermifuge. John Ellis named the Cape jasmine *Gardenia* in his honor. Garden was an ardent loyalist and never forgave his son who served in the Continental army. He

[7] *The Contest in America between Great Britain and France, by an Impartial Hand* (1757), and *The Present State of Great Britain, and North America* (1767).

[8] Copies of this map are contained in the collections of the Free Library of Philadelphia and the Historical Society of Pennsylvania.

[9] For an account of his life see Thacher's *American Medical Biography.*

[10] A century later Dr. William H. Welch said: "There was no more cultivated and attractive group of medical men in the third quarter of the eighteenth century in America than that in Charleston, South Carolina. Of these Bull was a pupil of Boerhaave, and Chalmers, Moultrie, Lining and Garden were trained in Edinburgh. These men were abreast of all the knowledge of the day. . . . Some were naturalists as well as physicians, their names being perpetuated in those of plants; they were Fellows of the Royal Society and correspondents of Linnaeus, Fothergill and other European savants." Most of these men had got their degrees at Edinburgh, where between 1768 and 1778 no less than ten South Carolinians had graduated in medicine.

[11] Elected in 1768 of Charleston.

[12] Chalmers published an "Account of Opisthotonus and Tetanus" in the *Transactions of the Medical Society of London,* 1754, and, in 1776, he published a *Treatise on the Weather and Diseases of South Carolina.* See his life in Thacher's *American Medical Biography.*

[13] Elected in 1768.

went over to London in 1783 and there he passed the last years of his life.[14]

Another Scotch physician who was an early member of the Society was the heroic Hugh Mercer[15] (1725–1777), who graduated M.D. at Aberdeen in 1744. In the following year he served in the army of the Pretender. After the disaster at Culloden he escaped to America, settling first at Greencastle, later called Mercersburg, in western Pennsylvania. He participated in Braddock's Expedition as captain of a company of Provincial soldiers, and was severely wounded at Fort Duquesne. He continued to serve as an officer in the subsequent warfare with the French and Indians. During this time he became a friend of Washington and at the conclusion of hostilities Washington persuaded him to settle in practice at Fredericksburg, Virginia, where his office is yet preserved. In 1776 he entered the Continental army, being commissioned by Congress a brigadier general. Mercer was mortally wounded at Princeton. He was attended by Benjamin Rush. His body was buried in Christ Church burial ground in Philadelphia. At a meeting of the American Philosophical Society, June 30, 1769, "A Letter on the Medical Uses of Apocynum" from Dr. Mercer was read.

As I have stated, a few native Americans went abroad to study in the first half of the eighteenth century, but for financial or other reasons returned without having received a degree. In this number were included Cadwalader Evans, Thomas Cadwalader and Thomas and Phineas Bond, all of whom were members of the Society. The fortunate ones who were able to study in a medical school and achieve the degree of M.D. included the following members of the Society, and it is noteworthy that seven of them graduated at Edinburgh, namely: Charles Moore, William Shippen, Jr., John Morgan, Adam Kuhn, Arthur Lee, Samuel Bard, Benjamin Rush. John Redman got his M.D. at Leyden; Hugh Williamson at Utrecht and John Jones at Rheims.[16]

During the Colonial Period Philadelphia was the medical center of the country and it is not local pride which renders it necessary to begin our aeroplane survey of the achievements of our medical members by recording some of the events which made medical history in this city, where the oldest hospital and the first medical school in America were founded. As early as 1730 or 1731 young Dr. Thomas Cadwalader[17] (1708–1779) returned from abroad where he had studied in London under William Cheselden, the famous surgeon and anatomist, and according to Dr. Caspar Wistar "made some dissections and demonstrations for the instruction of the elder Dr. Shippen and some others who had not been abroad, which are supposed to have been the first attempts to teach practical anatomy in the Colonies."

Dr. Cadwalader was the author of one of the earliest medical monographs published in this country, *"An Essay on the West India Dry Gripes,* to which is added an extraordinary case in physic. Philadelphia. Printed and sold by Benjamin Franklin. MDCCXLV." He was consulting physician to the Pennsylvania Hospital from 1751 to 1777.[18]

The Oldest Hospital in This Country

To the members of the American Philosophical Society is due the credit of founding the oldest hospital in the United States. It is the Pennsylvania Hospital which was founded in 1751.[19] Although its foundation is often ascribed to Benjamin Franklin, he distinctly disclaims the honor and ascribes the credit to Dr. Thomas Bond[20]

[14] Thacher's *American Medical Biography.*

[15] Elected in 1768.

[16] There remain twenty-three, out of the forty-eight members listed as physicians before 1768, of whose medical education I have been unable to learn anything. Some of them, like Dr. Ashton Warner and Dr. Adams, practiced in the West Indies and probably were educated in Great Britain but there are no details available concerning them.

[17] Elected in 1743.

[18] Watson in his *Annals of Philadelphia* tells a pleasant story of Cadwalader, illustrating his gracious manners and attractive deportment. In 1760 a Lieutenant Brulman of the Provincial militia became weary of the world and took the strange resolution to shoot the first person he met. He walked out "with a fusil in his hand" towards the Commons, which were then situated where the City Hall now stands, but where at that time game was abundant. He first met Dr. Cadwalader who bowed and said: "Good morning, sir, a fine day for your sport." Brulman afterwards said that though Dr. Cadwalader was an entire stranger to him there was something indescribable in his manner, which rendered him incapable of taking his life. Shortly afterwards the Lieutenant met Mr. Robert Scull who was not so fortunate as to please Brulman, who shot and killed him, for which crime he was duly executed.

[19] There are institutions, which are now hospitals but which were founded as almshouses or poor houses, before that date, such as the Philadelphia Almshouse, from which much later developed the Philadelphia Hospital, not known as such until 1824; the Charity Hospital of New Orleans, and Bellevue Hospital, New York.

[20] Elected in 1743, one of the leading physicians of Philadelphia.

(1712–1784). Bond had studied abroad but did not get a degree. He had a very large practice and was a skilful surgeon. Realizing the difficulty of operating and caring for patients in private houses or lodgings, he conceived the idea of establishing a hospital where they could be provided for, as the Almshouse only provided food and shelter for indigent citizens. He found it impossible to raise the necessary funds himself; everyone he approached asked him what Franklin thought of it. He, therefore, appealed to Franklin, who at once appreciated the value of such an institution and with his aid the Hospital was soon established. Thomas Bond and his brother, Phineas, and Lloyd Zachary were the first attending physicians. Phineas Bond (1717–1773) was also a member of the Philosophical Society.[21] He had studied abroad but, like his brother, had not obtained a medical degree. Both of the Bonds were trustees of the College of Philadelphia.[22]

In 1766, one year after the founding of the medical school at the College of Philadelphia, Dr. Thomas Bond began giving courses of clinical lectures to the students of the College at the Pennsylvania Hospital, the first clinical courses given in America. Bond's name is given to the present day to a form of splint used in the treatment of fractures of the forearm, the Bond splint.[23]

The Founding of the First Medical School in this Country

In 1765, fourteen years after the founding of the oldest Hospital in this country, the first medical college was established. All of the early teachers on its Faculty were members of this Society and as pioneers in organized medical teaching deserve mention.

This school was founded at the College of Philadelphia, now the University of Pennsylvania, in 1765. Early in that year Dr. John Morgan[24] (1735–1789) had returned to Philadelphia after some years spent in study abroad, during which he had acquired the degree of M.D. at the University of Edinburgh in 1763. He proposed to the Trustees the creation of a medical school in connection with the College, and they voted their approval and appointed Morgan Professor of the Theory and Practice of Medicine, in which capacity he delivered his famous "Discourse upon the institution of Medical Schools in America" at the Commencement exercises in May, 1765. It is difficult to realize how great an influence Morgan exercised on the development of American medicine on a high plane. He had had the best possible preliminary training, beginning with an apprenticeship under Dr. John Redman and as resident apprentice and apothecary in the Pennsylvania Hospital, he had served as surgeon with the Provincial troops in the French and Indian War. Going abroad he had studied with William and John Hunter in London, before going to Edinburgh. After graduating from the latter he had travelled on the Continent, where he had interesting meetings with Morgagni, the famous Italian pathologist at Padua, and with Voltaire at Ferney. Returning to Philadelphia he had brought with him a qualified English apothecary and announced that he would not put up medicines for his patients. He was an ardent patriot and, in 1775, was appointed Director General of the Hospitals and Physician-in-Chief of the Continental Army, succeeding the traitor Benjamin Church. Unfortunately his efforts to reform and improve the army medical service raised up enemies who succeeded in having him discharged by Congress in 1776. Subsequently he was vindicated by Act of Congress.[25] Morgan was physician to the Pennsylvania Hospital from 1773 to 1783.

William Shippen, Jr.[26] (1736–1808), was appointed Professor of Anatomy and Surgery in the medical school of the College of Philadelphia in September, 1765. He was well equipped for the position. Shippen began the study of medicine under his father Dr. William Shippen, Sr. In 1757 he went abroad and studied in London with the Hunters. While doing so he was particularly

[21] Elected in 1743.

[22] When the staff of the Pennsylvania Hospital was organized in 1751, in addition to Drs. Thomas and Phineas Bond and Dr. Lloyd Zachary as the attending physicians, the Board of Managers requested four physicians, Drs. Graeme, Cadwalader, Moore and Redman, all of whom were members of the American Philosophical Society, "to assist in consultations on extraordinary cases."

[23] Bond made a number of communications to the American Philosophical Society's *Transactions*. In 1754 he contributed to the *Medical Observations and Inquiries by a Society of Physicians in London* a report of a case of a worm in the liver, with the findings at autopsy, and in 1759 he sent the same Society through Dr. John Fothergill a paper on "The Use of the Bark in Scrophulous Cases."

[24] Elected in 1768.

[25] Morgan published a *Vindication* of his conduct, which is most valuable and interesting. In it he accuses his former friend and colleague Dr. William Shippen, Jr., who succeeded him in his position, of having been guilty of plotting against him and procuring his discharge.

[26] Elected in 1767.

favored with the friendship and assistance of Sir John Pringle, the Surgeon General of the British Army, Dr. John Fothergill, and the gifted young William Hewson, William Hunter's assistant. From London Shippen went to Edinburgh where he received his M.D. in 1761. After a short visit to France Shippen returned to Philadelphia in 1762. He brought with him a gift from Dr. John Fothergill to the Pennsylvania Hospital of some very valuable anatomical casts and paintings, which Fothergill wrote he hoped would be of use to Shippen in a school to teach anatomy and midwifery which he hoped to open in Philadelphia. Shippen started his school and continued it successfully until 1775 in spite of his teaching position in the College of Philadelphia.

When the University of Pennsylvania was organized by the union of the College of Philadelphia and the University of the State of Pennsylvania in 1791, Shippen continued to occupy the professorship of surgery and anatomy, with the addition of that of midwifery, with Caspar Wistar as adjunct professor. Shippen was physician to the Pennsylvania Hospital in 1778–79, but resigned on account of his military duties. He was re-elected in 1791 and served until his resignation in 1802. The controversy between Dr. Shippen and Dr. John Morgan was a most unfortunate affair.

Shippen was appointed by Congress to succeed Morgan as Director General of the Military Hospital and Physician-in-Chief of the Continental Army in 1777, and he held the position until his resignation in 1781. In 1780 he was courtmartialled on charges affecting his financial integrity, chiefly fomented by Dr. Benjamin Rush and Dr. Morgan. Shippen was acquitted and as stated above retained his position until 1781. Shippen was one of the founders of the College of Physicians of Philadelphia and its president from 1805 to 1808.

For several years Morgan and Shippen composed the entire medical faculty of the College of Philadelphia but, in 1768, the year in which its first class graduated, Dr. Adam Kuhn (1741–1817), who was elected a member of the Society in that year, was appointed Professor of Materia Medica and Botany and, in 1769, Dr. Benjamin Rush [28] was elected to the chair of Chemistry. Both of these men were graduated in medicine at the University of Edinburgh. Benjamin Rush (1741–1813) was one of the most remarkable men of his time. In 1789 he was appointed to succeed Dr. John Morgan as Professor of Medicine in the College and he continued to hold the chair until his death in 1813. During this period he was universally regarded as the leader of the medical profession in this country. From 1783 until his death he was physician to the Pennsylvania Hospital. He is said to have taught nearly three thousand students, and as he was an excellent and impressive teacher they spread his fame widely throughout the country. Dr. Rush was a member of the Continental Congress in 1776 and a Signer of the Declaration of Independence. During the Revolution he occupied several very important positions in the medical department of the Continental army. Unfortunately he became mixed up with Gates and Conway in their schemes to replace Washington in the command of the army.[29] Rush won undying fame for the courage and skill which he showed during the terrible epidemic of yellow fever in Philadelphia. His published account of the epidemic is one of the most interesting books of its kind, as vivid as Defoe's account of the plague in London, and less fictional. Although he justly claimed that he had treated more patients, and had been more successful in doing so, than any other physician in the city, one cannot help wondering why he was so successful as his chief reliance was placed on administering terrific doses of calomel and jalap, ten grains of each frequently repeated, and copious bleeding *ad deliquium animum*. His enemies, and he had them, dubbed him Dr. Sangrado.

Rush was a pioneer in the humane treatment of the mentally disordered, insisting that insanity was a disease and that many cases would respond to humane treatment.[30] Rush wrote much on the evils of alcohol and tobacco, and in favor of the emancipation of the negroes and the abolition of capital punishment. He was much interested in educational matters and was an important factor in founding two colleges in Pennsylvania, Dickinson and Franklin and Marshall.[31]

[28] Elected to the Society in 1768.

[29] In 1778 Rush published *Directions for Preserving the Health of Soldiers,* a very useful little book based on his army experience.

[30] In 1812 Rush published *Medical Inquiries and Observations upon the Diseases of the Mind,* the earliest work on mental diseases published in America.

[31] Rush's *Medical Inquiries and Observations,* first published in two volumes in 1789 and 1793, later enlarged to four volumes in many subsequent editions, contains the best of his work. An excellent portrait of Rush by Sully hangs in the library of the Pennsylvania Hospital.

The Medical School of King's College, New York

The second medical school founded in the Colonies was established in connection with King's College, New York, in 1767. Two of the four members of its faculty were members of the American Philosophical Society, Drs. John Jones and Samuel Bard, both very eminent in their profession. John Jones[32] (1729–1791) was born in Jamaica, Long Island. After studying medicine with Dr. Thomas Cadwalader in Philadelphia he went abroad and worked first in London under William Hunter and Percival Pott, then in Paris, and later in Edinburgh, finally getting his M.D. at Rheims in 1751. Returning to America he served as a surgeon with the Provincial soldiers in the war against the French and Indians. When the medical department of King's College was founded in 1767 Dr. Jones was elected Professor of Surgery. During the Revolution he was an ardent patriot but could not serve with the army in the field because of a severe asthma from which he suffered. He took an active interest, however, in organizing the army medical service and, in 1775, he published *Plain Remarks on Wounds and Fractures, designed for the Use of the Young Military Surgeons in America,* the first surgical textbook published by an American author. Dr. Jones' book was the standard work in use in the medical service of the Continental army throughout the Revolution. About 1780 Dr. Jones left New York to reside in Philadelphia, where he found he suffered less from asthma than elsewhere. In that year he was elected to the staff of the Pennsylvania Hospital, on which he served until his death. When the College of Physicians was founded, in 1787, he was elected its first Vice-President. Dr. Jones was Benjamin Franklin's physician and left an interesting account of his last illness and, in 1790, he was called to New York to see President Washington, who was desperately ill. His recovery was attributed to Dr. Jones' advice and when the seat of the federal government was moved to Philadelphia, Washington chose him as his family physician.

Dr. Samuel Bard[33] (1742–1821) was born in Philadelphia. When Samuel was four years old his father, the distinguished Dr. John Bard, moved to New York. Later Samuel began the study of medicine with his father, and in 1760 he went abroad. After studying at St. Thomas' Hospital, London, he went to Edinburgh, where he graduated in medicine in 1768, in which year he returned to New York and was appointed Professor of the Theory and Practice of Medicine in King's College. Bard was a Tory and during the Revolution spent most of the time in retirement in New Jersey. After the war was over he resumed practice in New York. His loyalist proclivities do not seem to have been held against him. When the federal government was in New York he was Washington's family physician.

The Revolution disrupted King's College and its medical school. When the institution was revived as Columbia College, in 1792, Dr. Bard resumed his position in the faculty. Dr. Bard published much on medical subjects. He was deeply interested in agriculture and, in 1811, published *A Guide for Young Shepherds.*

The title of the "Father of American Surgery" has been accorded, not without reason, to Philip Syng Physick[34] (1768–1837) of Philadelphia. In spite of the boy's repugnance to medicine his father insisted that he should study it. He began his studies under Dr. Adam Kuhn, and it is recorded such was his sensitiveness that he fainted when the latter took him to see an amputation. In 1788 Physick went abroad where he became a special student with John Hunter and, through his good offices, secured the position of house surgeon in St. George's Hospital for a year. At the expiration of that time he received a diploma from the Royal College of Surgeons of London and John Hunter asked him to come and live with him as his assistant. This Physick did for a few months before his departure in May, 1791, to study in Edinburgh, from which University he received the degree of M.D. in May, 1792. In the autumn of that year he returned to Philadelphia and began to practice. During the terrible epidemic of yellow fever in the following year, Physick served as physician in the yellow fever hospital at Bush Hill. In this capacity his courage and skill won him great praise and, fortunately for Physick, attracted the special attention of Stephen Girard. The latter had a strong prejudice against physicians in general, but made an exception in favor of young Physick, which proved of value to him throughout his life. From 1794 to 1831 Physick was Physician to the Pennsylvania Hospital[35] and the records of the Hos-

[32] Elected to the Society in 1769.

[33] Elected to the Society in 1768.

[34] Elected to the Society in 1802.

[35] Until 1823 the members of the staff of the Pennsylvania Hospital were all elected as *Physicians.* In May,

pital indicate what a great surgeon he was. He was especially successful in his treatment of fractures and dislocations. In those days the scope of the operative surgeon was much more limited than it became after the introduction of anaesthesia and antisepsis. The interior of the three great cavities of the body; the skull, the chest and the abdomen, were rarely invaded by the surgeon and his activities were largely confined to operations for aneurism; the removal of tumors, most frequently termed wens; amputations; the operation for cataract; and the removal of stones in the bladder. In the two latter operations Dr. Physick particularly excelled. When he was sixty-three years old, in October, 1831, Physick operated for stone in the bladder on Chief Justice John Marshall, then aged seventy-six years, successfully removing upwards of one thousand calculi from his bladder. In 1812 Dr. Physick washed out the stomachs of two children who were suffering from an overdose of opium; one child died but the other recovered. This was the first time lavage was ever employed for the removal of ingested poison. In 1816 Physick published[36] an account of a method he had devised for making ligatures out of animal tissues which he employed until the time of his death. In 1820 Physick published[37] a method of removing the tonsils by an instrument consisting of a double cannula with a flexible wire and, later, he devised a guillotine knife to be used for the removal of the uvula or the tonsils. In 1805 Physick was elected Professor of Surgery in the University of Pennsylvania and remained so until his resignation in 1820.

Dr. Samuel D. Gross[38] speaks rather disparagingly of Physick whom he heard lecture on several occasions. Gross says that though he was a man of marked ability, "that he possessed talents that towered above those of all his professional contemporaries is not true. Caldwell, Chapman, Drake and Hosack, not to mention others, were men of vastly greater mental calibre." Gross considered that he was a very poor lecturer and that in his manners he was cold and unsociable; that he kept himself aloof from the world apparently to enhance his greatness. "Yet Physick was at heart a kind man, full of sympathy for suffering humanity."

1823, the Minutes first record the division in physicians and surgeons. In 1810, however, Thomas Chalkley James had been elected to the Lying-in Department.

[36] *Electic Repository*, Vol. VI, 1816.

[37] *Philadelphia Journal of the Medical and Physical Sciences*, 1, 1820.

[38] *Autobiography*, 11, 240 et sq.

Anatomists

The earliest treatise on anatomy published in this country was by Dr. Caspar Wistar (1761–1818), of Philadelphia. Wistar began the study of medicine under Dr. John Redman and at the College of Philadelphia from which he received the degree of bachelor of medicine in 1782. He went abroad, studied in London for a year and then went to Edinburgh, where he obtained the degree of M.D. in 1786. In 1789 he was appointed to the professorship of chemistry in the College of Philadelphia, and when that institution was united with the University of the State of Pennsylvania to form the University of Pennsylvania in 1792, Wistar was appointed Adjunct Professor of Anatomy, Surgery and Midwifery to William Shippen, Jr. When Dr. Shippen died in 1808, Wistar was appointed Professor of Anatomy. From 1793 to 1810 he was physician to the Pennsylvania Hospital. Dr. Wistar's most original contribution to anatomy was his elucidation and demonstration of the posterior ethmoids, and their relation to the sphenoid bone. From 1811 to 1814 he was occupied in bringing out his *System of Anatomy*, the first work of that kind published in America. Dr. Wistar was elected a member of the American Philosophical Society in 1787, and succeeded Thomas Jefferson as its President in 1816. For some years Wistar kept open house on a certain evening of the week for the members of the Society and any persons of learning or distinction whom they might bring with them. After his death a group of his friends organized the "Wistar Party," an association which exists to the present day. Once a month during the winter season a party is given by one of its members, all of whom belong to the American Philosophical Society. The invitations, as Thackeray who was a guest on one occasion remarked, still bear an engraved representation of the "pigtailed Whister" (sic). His name is also commemorated in the beautiful vine, the *Wisteria*, named for him by the naturalist Thomas Nuttall. At his death his anatomical collection was left to the University of Pennsylvania. To it was subsequently added the collection of Dr. Horner. In 1892 Wistar's nephew General Isaac Wistar endowed the Wistar Institute of Anatomy and Biology, incorporating these collections.

William Edmonds Horner[39] (1790–1853) was

[39] Elected 1819.

born in Warrenton, Virginia. He began his medical studies under Dr. John Spence, an Edinburgh graduate who practiced in Virginia. In 1813 Horner enrolled as a medical student in the University of Pennsylvania. While still a student he was appointed as a surgeon's mate in the army and served in northern New York during the summer of 1813. Returning to the University in the autumn he got his M.D. in April, 1814, and was commissioned as a surgeon in the army. In this capacity he saw very active service about Lake Erie. When peace was concluded Horner resigned from the army. The winter of 1815–1816 he lived in Philadelphia devoting much time to the study of anatomy at the University of Pennsylvania. His zeal in so doing attracted the attention of Dr. Caspar Wistar, the professor of anatomy, who appointed him his dissector in March, 1816. This was the beginning of a life-long friendship between the two men. When Wistar died, in 1818, he was succeeded by John Syng Dorsey who appointed Horner demonstrator of anatomy. Dorsey died shortly after delivering his introductory address, and Philip Syng Physick took the professorship of anatomy. Physick kept Horner on as demonstrator of anatomy. In 1820 Horner was made adjunct professor, and when Physick resigned in 1831, he succeeded him as full professor. Horner was one of the first physicians in this country to use the microscope in his researches. In 1832 he studied the microscopic lesions in the mucosa of the intestinal tract in cholera, and he showed that the glands in the axilla of the negro were larger and more numerous than they were in the white man. In 1824 Horner published in the *American Journal of the Medical Sciences* his discovery of a small muscle, the "tensor tarsi," situated on the posterior surface of the lachrymal apparatus. Although the muscle had been noticed by others its exact structure and relationship had never been so fully or accurately described. Horner edited an edition of Wistar's *Anatomy* and published several other books on that subject. He wrote also many articles for the medical journals. Gross[40] says: "Horner was the most accomplished anatomist that our country has produced." He made a wonderful collection of anatomical specimens at the University of Pennsylvania. These were housed with the collection previously left by Dr. Caspar Wistar, forming the Wistar and Horner Museum, now included in the Wistar Institute of Anatomy at the University.

[40] *Autobiography*, 11, 274.

Medical Education in New England

Although there were many excellent practitioners of medicine in New England during the Colonial period, there were no hospitals, except pest houses and almshouses, and no medical schools until after the end of the Revolution.[41] Just as elsewhere, many of the best doctors had received no training except as an apprentice to another doctor. Some had studied abroad as did Dr. James Lloyd (1728–1810) who was elected a member of the American Philosophical Society in 1770, and was the most eminent practitioner and had more pupils than any other physician in New England. After studying for five years under Dr. William Clarke, of Boston, Lloyd went over to London where he spent two years as a dresser in Guy's Hospital and attended the courses given by Dr. Smellie and Dr. William Hunter on obstetrics and anatomy. In 1790 Harvard conferred on him an honorary M.D.

There were two others of the members of the Society from New England who are listed as physicians, who had no medical education beyond that received during their apprenticeship to established practitioners yet both achieved professional eminence. They were Benjamin Gale (1715–1790), elected in 1768, and Edward Augustus Holyoke (1728–1829), likewise elected in 1768.[42]

Harvard Medical School and the Introduction of Ether Anaesthesia

In 1780 Dr. John Warren, a younger brother of the hero Dr. Joseph Warren who was killed at Bunker Hill, gave courses on anatomy at a military

[41] The Massachusetts General Hospital received a charter in 1811, and was not opened for the reception of patients until 1821.

[42] Gale practiced in Killingworth, Connecticut. He wrote several essays on medical subjects which were published in the *Transactions* of the Medical Society of New Haven and, in 1750, published a *Dissertation on the Inoculation of the Smallpox* in America, which was referred to by the celebrated English physician Dr. Huxham. He also was a biblical scholar and published a *Dissertation on the Prophecies*. Gale received a medal from an English society for his invention of a drill plough. Holyoke practiced in Salem, Massachusetts, throughout his long life. He graduated from Harvard College in 1746. His father, Edward Holyoke, was President of Harvard from 1739 to 1769. After serving a two years' apprenticeship with Dr. Berry, of Ipswich, Holyoke began practicing. In this he was very successful and had a number of pupils as well. In 1781 he was elected first president of the Massachusetts Medical Society. In 1783 Harvard conferred on him the honorary degree of M.D.

hospital in Boston, and two years later, in 1782, the Corporation of Harvard College established a medical school in connection with that institution. The faculty was composed of Dr. John Warren, Professor of Anatomy and Surgery; Dr. Benjamin Waterhouse, Professor of the Theory and Practice of Physic, and Dr. Aaron Dexter, Professor of Chemistry.

Dr. Benjamin Waterhouse (1754–1846) was elected a member of this Society in 1791.[43] Waterhouse studied in London and Edinburgh but got his M.D. at Leyden in 1781. He was one of the first physicians to introduce vaccination into this country. Waterhouse was an ardent student of natural history. He cultivated an excellent botanical garden, and obtained from Dr. John Coakley Lettsom, of London, a fine collection of minerals which formed the nucleus of Harvard's mineralogical collection.

Dr. John Warren was the first of what was frequently called the "Warren Dynasty," which until recent years occupied a preëminent place in the medical profession in Boston. His son John Collins Warren[44] (1778–1856) began his medical studies with his father. In 1799 he went abroad, became a pupil of Sir Astley Cooper and served as a dresser in Guy's Hospital, London. After graduating M.D. at Edinburgh, Warren went to Paris where he studied under Corvisart and Dupuytren. On his return to Boston, in 1802, he was elected adjunct professor of surgery and anatomy at Harvard and, on his father's death in 1815, succeeded him in the full professorship. On October 16, 1846, in the operating room of the Massachusetts General Hospital Dr. Warren performed the first operation in public under ether anaesthesia, an event ever since annually celebrated as "Ether Day" at the Hospital. Dr. Warren's fine anatomical collection formed the nucleus of the Warren Museum at Harvard.[45]

[43] Although the statement is frequently made that his mother, Hannah Proud, was a niece of Dr. John Fothergill, it is an error. She was Fothergill's first cousin. Her husband was Timothy Waterhouse, of Newport, Rhode Island. See *Dr. John Fothergill and his Friends*, by R. Hingston Fox.

[44] Elected to the Society in 1818.

[45] His son Dr. Jonathan Mason Warren (1811–1867) assisted Dr. Warren in the performance of the first operation in which the value of ether anaesthesia was publicly demonstrated. He went abroad in 1832 and spent some years studying in London, Dublin and Paris. In the latter city he worked under Louis with Gerhard, Peace and Pepper, from Philadelphia, and Jackson, Bowditch and Holmes, of Boston. He was a very able surgeon, especially renowned for his skill in operating for cleft palate. He was surgeon to the Massachusetts General Hospital. His son John Collins Warren carried on the family tradition, being one of the most noted surgeons of his day.

[46] Elected to the Society in 1832.

The Influence of the Americans Who Studied at Paris on the Teaching and Practice of Medicine in the United States, Marking the Beginning of Scientific Medicine in this Country

During the first half of the nineteenth century Paris was the great center for medical teaching. It is only necessary to mention the names of some of the great French masters of medicine who flourished in Paris at that time to understand why postgraduate students flocked there from all over the world; Bichat, Corvisart, Laennec, Chomel, Andral and especially Louis, the favorite teacher of the American students, were the leaders in medicine and pathology, teaching the new methods of diagnosis, percussion revived by Corvisart, and mediate auscultation discovered by Laennec. In surgery Dupuytren, Larrey, Velpeau, Malgaigne and Ricord were a group of great operators and teachers. Many of their American pupils brought back their methods and ideas to the United States, so that the Edinburgh and London traditions of the eighteenth century yielded place to the French, until they in turn were displaced by the rise of the German medical science under Virchow and Koch and their disciples.

In 1819 Laennec published at Paris his great book *L'Auscultation Mediate* in which he described his discovery of the stethoscope, an instrument by which the physician could hear and interpret the sounds produced by the heart and lungs in health and disease. Laennec was a popular teacher and had many pupils but the majority of the young Americans who studied in Paris and had so much influence on medical teaching in this country, went there after Laennec's death, which occurred in 1826. Two members of this Society were pupils of Laennec, however, and had much to do with introducing into the United States Laennec's method of listening over the chest with a stethoscope. John Bell[46] (1796–1872), a native of Ireland, came to Philadelphia in early life. He got his M.D. at the University of Pennsylvania in 1817, and then went to Paris for postgraduate work, chiefly under Chomel, Andral and Laennec. On his return he practiced in Philadelphia and gave lectures in the Medical Institute on the insti-

tutes of medicine. Later he went out West and became professor of that subject in the Medical College of Ohio. He not only taught his students the use of the stethoscope, but edited in 1840 an American edition of Stokes' *Lectures on the Theory and Practice of Medicine,* a book which greatly influenced the spread of the knowledge of Laennec's discovery.

The other member of the Society who had been a pupil of Laennec was Samuel George Morton[47] (1799–1851) of Philadelphia, who studied medicine with Dr. Joseph Parrish and got his M.D. from the University of Pennsylvania in 1820. Subsequently he went abroad and studied for some years in Edinburgh and Paris. Returning to Philadelphia in 1824, he gave lectures on anatomy and worked at the Academy of Natural Sciences. He developed an intense interest in craniology, forming the wonderful collection of skulls which furnished him the material for his two great works, *Crania Americana* and *Crania Egyptica.* In 1834 he published *Illustrations of Pulmonary Consumption,* of which Osler[48] said it was "The earliest and one of the best books on phthisis written in America by a pupil of Laennec."

One of the most distinguished of the young men from Philadelphia who studied at Paris in the first half of the nineteenth century was William Wood Gerhard[49] (1809–1872), who graduated from the medical department of the University of Pennsylvania in 1830, and then went over to Paris, where he studied under Chomel, Andral and Louis. After three years in Paris Gerhard returned to Philadelphia and served a term as resident physician in the Pennsylvania Hospital. Shortly afterwards he was elected to the staff at the Philadelphia Almshouse hospital. As typhus prevailed in Philadelphia in 1836, Gerhard had an opportunity to study the disease and to contrast it with typhoid fever, with which it had, heretofore, been confused. In 1837 he published a series of papers in the *American Journal of the Medical Sciences,* in which the two diseases were clearly differentiated. In 1842 Gerhard published a book, *The Diagnosis, Pathology and Treatment of Diseases of the Chest,* in which the value of the stethoscope as a help to diagnosis was clearly proved and the adoption of Laennec's recent work on auscultation was established in the United States. From 1845 to 1868 Gerhard was physician to the Pennsylvania Hospital.

[47] Elected to the Society in 1828.
[48] Quoted by Lawrason Brown.
[49] Elected to the Society in 1843.

William Pepper[50] (1810–1864), the first of a distinguished line of physicians of that family, graduated M.D. at the University of Pennsylvania in 1832. After distinguishing himself by his work at the Bush Hill Hospital during an epidemic of cholera in that year, Pepper went abroad and studied for two years in Paris, chiefly under Dupuytren and Louis. He was physician to the Pennsylvania Hospital from 1842 to 1858. In 1860 he succeeded Dr. George B. Wood as Professor of the Theory and Practice of Medicine in the University of Pennsylvania. Dr. Pepper was an excellent didactic teacher and made many important contributions to periodical medical literature. His son William Pepper, Jr. (1843–1898), likewise occupied the chair of medicine in the University of Pennsylvania and was also provost of that institution.[51] Provost Pepper was not only a great teacher, medical consultant, and administrator, but he was animated by an undaunted public spirit. To him more than to any other individual Philadelphia owes the creation of her Commercial Museum, the great Free Library, and the Museum of the University of Pennsylvania.

One of the most distinguished of the New Englanders among the group in Paris was Oliver Wendell Holmes (1809–1894). Dr. Holmes was not elected to the Society until 1880, but from our point of view he should be considered with those who were his fellow students in Paris. Holmes graduated from Harvard College in 1829 and then studied medicine at Harvard for several years before going abroad and spending three years in Edinburgh and Paris. He returned to Boston and got his M.D. at Harvard in 1836. He won several prizes in the course of the next year or so by writing essays, one of which, on the *Utility and Importance of Direct Exploration in Medical Practice,* deserves special mention because it did much to further the introduction and use of percussion, and the recently introduced mediate auscultation of Laennec, into medical practice in the United States. In 1843 Holmes read before the Boston Society for Medical Improvement a paper "On the Contagiousness of Puerperal Fever," in which he stated that the disease was conveyed by the hands of the physician or nurse, or by dirty dressings.[52]

[50] Elected in 1851.
[51] Elected to the Society in 1870.
[52] Holmes' paper was published in the *New England Quarterly Journal of Medicine and Surgery,* 1842–43, 1, 503–530, a medical journal which had a very limited circulation.

This was four years before Semmelweis, of Vienna, published his classic paper stating the same views. Holmes' statements aroused the most furious opposition, particularly by Dr. C. D. Meigs and Dr. Hugh L. Hodge.

In 1847 Holmes was appointed Parkman Professor of Anatomy and Physiology at Harvard. A special professorship of physiology was created in 1871 but Holmes held the chair of anatomy until he resigned in 1882. He was a very popular teacher. Curiously enough, though he brought a microscope home with him from Europe and was much interested in working with it, Holmes never used the microscope in his teaching and he made no discoveries or advances in anatomy. With his literary fame we are not at present concerned. He ranks higher than any other medical man in American literature.

On their return from their fruitful studies under the great French teachers of the early years of the nineteenth century these young men introduced scientific medicine into the United States.

JOSEPH PRIESTLEY AND THE EARLY HISTORY OF THE AMERICAN PHILOSOPHICAL SOCIETY

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(*Read February 13, 1942, in Symposium on the Early History of Science and Learning in America*)

JOSEPH PRIESTLEY's claim to an entire paper in this series cannot be justified by the length or the importance of his scientific labors in America. His appeal to the interests of the members of the American Philosophical Society is more intimate and personal.

Priestley's scientific career and our Society had a common origin in the enthusiasms of Franklin. For Priestley attributes the original stimulus for his philosophical investigations to a meeting with our founder, and the early course of his scientific and political thinking was largely influenced by his friendship with Franklin. In turn Priestley's distinguished chemical researches, performed while in England, provided the basis for many of the discussions in the early meetings of the Society. The high regard for Priestley that developed among our members as a result of these reports gained him election to foreign membership nine years before his arrival in America. Finally, in 1794, the American Philosophical Society welcomed him to its more immediate fellowship and became the focus of his scholarly associations during the ten years of his exile. Because of these relations it is appropriate that this paper should be more concerned with the character and influence of the man and less with a recital of his scientific observations.

In a letter to Josiah Wedgwood, dated 23 March, 1783, Priestley says, "This day I complete my half century," thus placing his birth in the year 1733. His immediate ancestors were reputedly substantial yeomen, industrious and noted for their religious zeal in the dissenting sects. From such a background he derived his practical and liberal outlook on affairs, his extraordinary industry, his life-long devotion to theology and moral problems.

Despite the impediment of a marked stammer which he never completely overcame, he prepared for a career as a minister by attending Daventry Academy for three years. Entries in his diary of this period reveal the beginnings of his wide range of interests and the orderly habits of his work to which he attributed his prolific accomplishments. After three years he graduated from the Academy "with a belief, more or less qualified, of the doctrine of atonement" and began his undistinguished career as a preacher to a small congregation at Needham Market in Suffolk. Three years in that position were marked by no accomplishment of note other than the publication of a treatise entitled, "The Doctrine of Remission." And this is notable only because it is the first of his voluminous publications that ranged over many subjects and fill, in a collected edition, no fewer than forty-five large and closely-printed volumes. His ink-pot career was begun, and it did not end until his death-bed, where his son reports his last act to have been the correction of a proof-sheet.

His next move, which was to Nantwich, was a happier one. There he found a more sympathetic congregation and ample leisure to conduct a school in which he revealed his remarkable talents as a teacher. This circumstance was of great significance in his scholarly career for it brought him to the attention of the Warrington Academy. This was at the time the leading Nonconformist academy and a center of scholarship that had much influence on English education and science. To that intellectually stimulating environment Priestley was called in 1761 as Tutor in Classical Languages and Polite Literature. He confesses, however, that he would "have preferred the office of teaching the Mathematics and Natural Philosophy, for which I had at that time a great predilection."

Most of his earlier publications during the period at Warrington deal with theology and education. Of especial interest to us is his "Essay on Education" published in 1764. In this appeal for a more practical and realistic educational system he urged that the instruction of youth should be adapted to the circumstances and needs of the time in which they live, and that one "should con-

trive that the studies of youth should tend to fit them for the business of manhood." There is much in this treatise that is reminiscent of Franklin's ideas on education, published some years before in his "Proposals Relating to the Education of Youth in Pennsylvania."

Similar views on such subjects and a wide-ranging curiosity in all matters must have provided a basis for mutual friendship when Priestley and Franklin met in 1765. Shortly before that time Priestley had formed the custom of going to London for a month each year for an intellectual tonic. On such an occasion he was introduced to Franklin and told our founder of an idea which he had entertained of writing a history of the discoveries in electricity. "This I told Dr. Franklin might be an useful work and that I would willingly undertake it, provided I could be furnished with the books necessary for the purpose. This he, Franklin, readily undertook, and I set about the work without having the least idea of doing anything more than writing a distinct and methodical account of all that had been done by others. Having, however, a pretty good machine I was led in the course of my writing the history, to endeavor to ascertain several facts which were disputed; and this led me by degrees into a large field of original experiments in which I spared no expense that I could possibly furnish." Thus did Priestley cease to be merely a teacher and literary jack-of-all-trades and become an "experimentarian philosopher," as some of those who scorned his theoretical abilities dubbed him.

Among the "original experiments" was one suggested by Franklin and from which Priestley is said to have drawn the brilliant inference that the attraction of electrified masses follows the law of gravitational attraction, *i.e.*, the inverse square law. These researches were the means of introducing him into the Royal Society on the recommendations of Dr. Franklin and three others. Priestley's signature stands third on the page for the year 1766 in the Charter Book of our Sister Society. Thus started by Franklin on his career as an experimental philosopher, Priestley never forsook the joys of the laboratory despite grave handicaps.

In another respect Franklin found Priestley a willing disciple. This was in the field of liberal political thought to which he contributed a long series of controversial pieces. Some of these dealt with the strained relations between England and the American colonies and are thought to have been encouraged by Dr. Franklin. In any event, Priestley was a loyal friend to Franklin throughout the time the latter was suffering humiliating treatment at the hands of the Privy Council. On the last day but one of Franklin's stay in London in March 1775 he received Priestley and they talked about science, as if they were to meet again the next day. "But they found little pleasure in their conversation for the newspaper on the table was filled with the miseries of Massachusetts, now occupied by the British Army. Together they read the warlike declarations and their eyes filled with tears. Of what use was intelligence and patience. The time for negotiations was over and Franklin had to go." Fortunate for Priestley that he could not look ahead twenty years and see that he would follow Franklin to Philadelphia under circumstances no less humiliating.

At this time Priestley's career as a scientist was in full course. Following his teaching at Warrington Academy and a subsequent congregation at Leeds he had become librarian and companion to Lord Shelbourne. The acceptance of this position was an important epoch in his scientific career for it provided him with leisure and facilities for research. It was then that he began his remarkable contributions relating to the production of various gases, their composition and properties. Early in this series of investigations he described in 1772 a method for impregnating water with fixed air or carbon dioxide. It was well considered by the Admiralty as a cure for scurvy and for this reason, or others more pleasant, Priestley was awarded the Copley Medal of the Royal Society in 1773 as the discoverer of carbonated water.

Of especial significance are the biological experiments he performed in that same year which demonstrated the reciprocal relation between animal and plant life. He found that air in which a candle had burned until it went out spontaneously again became respirable and capable of supporting combustion after plants had grown in it for some time. This recognition of the relation between combustion and respiration he described as follows:

> That candles will burn only a certain time in a given quantity of air is a fact not better known than it is that animals can live only a certain time in it; but the cause of the death of the animal is not better known than that of the extinction of flame in the same circumstances; and when once any quantity of

air has been rendered noxious by animals breathing in it as long as they could, I do not know that any methods have been discovered of rendering it fit for breathing again. It is evident, however, that there must be some provision in Nature for this purpose, as well as for that of rendering the air fit for sustaining flame; for without it the whole mass of the atmosphere would, in time, become unfit for the purpose of animal life; and yet there is no reason to think that it is, at present, at all less fit for respiration than it has ever been. I flatter myself, however, that I have hit upon two of the methods employed by Nature for this great purpose. How many others there may be I cannot tell.

He subsequently proved that this "Melioration of Air by the Growth of Plants" is dependent on the green matter of leaves and upon sunlight.

On the first of August 1774 Priestley performed the experiment that earned for him immortal fame. He reports it as follows:

I endeavored to extract air from mercurious calcinatus, and I presently found that by means of my burning lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what surprised me more than I can well express was that a candle burned in this air with a remarkably vigorous flame.

The properties of the oxygen which he had prepared for the first time he ultimately described with accuracy thus:

I easily conjectured that inflammable air would explode with more violence and a louder report by the help of dephlogisticated than of common air; but the effect far exceeded my expectations, and it has never failed to surprise every person before whom I have made the experiment. . . . The dipping of a lighted candle into a jar filled with dephlogisticated air is alone a very beautiful experiment. The strength and vivacity of the flame is striking, and the heat produced by the flame in these circumstances is also remarkably great. . . . Nothing would be easier than to augment the force of fire to a prodigious degree by blowing it with dephlogisticated air instead of common air. . . . Possibly platina might be melted by means of it.

From the greater strength and vivacity of the flame of a candle, in this pure air, it may be conjectured that it might be peculiarly salutary to the lungs in certain morbid cases. . . . But perhaps we may also infer from these experiments that though pure dephlogisticated air might be very useful as a medicine, it might not be so proper for us in the usual healthy state of the body: for, as a candle burns out much faster in dephlogisticated than in common air, so we might, as may be said, live out too fast, and the animal powers be too soon exhausted in this pure kind of air. A moralist, at least, may say that the air which Nature has provided for us is as good as we deserve. . . . Who can tell but that, in time, this pure air may become a fashionable article in luxury. Hitherto only two mice and myself have had the privilege of breathing it.

He was, however, very far indeed from a clear realization of the true significance of what he had discovered. He called it dephlogisticated air, thinking it was the component of the atmosphere with which the phlogiston united when it emerged from a burning substance. He called nitrogen "phlogisticated air," and this nomenclature would seem to imply that he considered it a product of the union of the oxygen with the phlogiston. If so, nitrogen should sometimes appear as a product of combustion, but this contradiction was overlooked together with every other fact that told against the phlogiston theory. Priestley was so sure that something was always given off in combustion that he lost the power to believe that the burning body united with one of the components of the atmosphere.

His adherence to the phlogiston doctrine likewise confused him regarding the respiratory functions of the blood and the lungs, described by him to the Royal Society in 1775 in a paper on "Respiration and the Use of the Blood." He believed the function of the lungs was "to carry off a putrid effluvium, or to discharge that phlogiston, which had been taken into the system with the aliment, the air that is respired serving as a menstruum for that purpose." This he thought he had "proved to be effected by means of the blood, in consequence of its coming so nearly into contact with the air in the lungs, the blood appearing to be a fluid wonderfully formed to imbibe and part with that principle which the chemists call phlogiston, and changing its color in consequence of being charged with it or being freed from it."

The first recorded presentation of Priestley's researches to our Society is on the 19th of March 1784 when S. Vaughan read the first part of a manuscript entitled, "Phlogiston and the Seeming Concoction of Water into Air." This is apparently the record of Priestley's experiments upon which James Watt subsequently based his conjectures concerning the chemical nature of water. This use of Priestley's results earned for Watt the credit for this discovery in association with Cavendish and Lavoisier, but Priestley was again

confused by the intricacies of the phlogiston theory. I cannot find that he ever attained a true understanding of the important significance of these experiments which he had performed.

Throughout the period of his greatest scientific activity Priestley was preparing discourses and pamphlets on controversial religious or political questions, and following the severance of his relations with Lord Shelbourne he took a Unitarian congregation in Birmingham. The instincts for civil liberty that had made him a willing disciple of Franklin made him sympathetic to the principles of the French Revolution. The knowledge of this by the general public, his outspoken opposition to some of the leading members of the Government, and his position as a Unitarian preacher combined in arousing antagonism to him in Birmingham. This animosity ultimately led to the Birmingham riots on Bastille Day 1791 which were primarily directed against Priestley and some of his more intimate associates. His house and laboratory were destroyed, and most of his library and experimental apparatus. Concerning what cause he may have given in provoking the sentiment for these riots the opinions are many, but there seems to be little ground for thinking him a disagreeable or combative man. Franklin certainly speaks of him in no such terms, for in writing to Vaughan in 1788 he says, "Remember me affectionately to the honest heretic, Dr. Priestley. . . . Do not however mistake me. It is not to my good friend's heresy that I impute his honesty. On the contrary it is his honesty that has brought on him the character of heretic."

The rude ejection from Birmingham, the threats against him in London and the lack of sympathy even within the Royal Society finally caused Priestley to withdraw from the Society and sail for America. While he and his wife were at sea, leaving their country because of intolerance, his scientific protagonist, Lavoisier, was executed because, in the terms of the sentence, "the Republic had no need of men of science."

In the minutes of our Society for 20 June 1794, there is this paragraph:

> Congratulatory address to Dr. Priestley. Rittenhouse, Mr. Barton, Thornton, Bleakley, Duponceau [sic], Williams, a Committee to draft it. This Committee reported a draft; it was read and adopted "and the Officers of the Society with as many of the other members as can conveniently attend are directed to meet at the Hall to-morrow afternoon at one o'clock in order to present the same."

Thus began a period of ten years during which Priestley was intimately associated with the affairs of our Society. He attended meetings from time to time. He was considered for president but declined in favor of Mr. Jefferson. The publication of his scientific papers was divided between our *Transactions* and the *Medical Repository*.

Although Priestley was not at first invited to preach as much as he desired, he was in other respects made welcome in the life of the community from the very beginning of his exile. Soon after his arrival in Philadelphia, he was offered the professorship of chemistry at the University of Pennsylvania. He was tempted by the opportunities life in Philadelphia would have offered for scientific research and for establishing a Unitarian congregation. But he pleaded that although he had made discoveries in some branches of chemistry he had never given much attention to the common routines and knew but little of the common processes. The more complete explanation for his refusal of this professorial post is that he did not at first like Philadelphia with its "too obviously prosperous Quakers and the summer heat." He accordingly followed his original intention of joining his sons who had some years before settled at Northumberland on the upper banks of the Susquehanna.

Here the pattern of life and the range of Priestley's interests were much the same as they had been in England. Although he had declined the opportunity to found a school of chemistry at Pennsylvania he lost none of his former concern with the problems of education. He was soon engrossed in plans for the establishment of an academy or college at Northumberland, and the venture went somewhat towards realization. There is also correspondence between him and Jefferson with advice for the latter regarding the organization of the University of Virginia, and an expression of Priestley's wish that he were younger so that he might become one of the faculty.

His opportunities for preaching in the rural region of Northumberland were naturally limited, but he continued his religious writings on the usual extensive scale, completing at this time his "Church History from the Fall of the Western Empire to the Reformation." In the spring of 1796, he returned to Philadelphia where he delivered a series of lectures on "The Evidences of Revelation" to crowded audiences, and this evidently pleased him.

During this visit the minutes of the American Philosophical Society report his frequent presence at the meetings and his participation in the discussions. There is abundant evidence that he gave vigorous stimulus to scientific deliberations, and he was certainly held in warm respect by his fellow members. Thus there is to be found in the minutes for 28 February 1803:

> Resolved: That this Society will dine together on Saturday next, and that J. B. Smith, Wistar, Williams, Hewson and Vaughan be a Committee to make the necessary arrangements for that purpose and to request Dr. Priestley's company, informing him that the Society are induced to make the request from their high respect for his Philosophical Labors and discoveries, and to enjoy the more particular pleasure of a social meeting.

The "high respect for his Philosophical Labors and discoveries" was, however, owing to his earlier work. For although he continued his labors in the little laboratory at the home in Northumberland the accomplishments were not among the more notable of his career. The discovery of carbon monoxide was made, it is true, in this period, but the tempo of his scientific life had slowed down, and relatively more of his time was devoted to meditation and writing.

Unfortunately the increased time for meditation did not clarify his thoughts regarding his previous research and enable him to organize his illustrious chemical discoveries into the pattern of ideas which was by then generally recognized as correct and consistent. As late as 1800 he put together his "Doctrine of Phlogiston Established," and wrote to a friend: "I have well considered all that my opponents have advanced, and feel perfectly confident of the ground I stand upon. . . . Though nearly alone I am under no apprehension of defeat." It must, however, be recorded to his credit that he knew his weakness for taking the wrong end in his theoretical deductions, as well as his strength as an experimentalist and observer. "It is not my opinions on which I would be understood to lay any stress," he once said. "Let the *new facts,* from which I deduce them be considered as my *discoveries,* and let other persons draw better inferences from them if they can."

On the 6 February 1804, Priestley died at Northumberland, among the hills of Pennsylvania. It was not unfitting that his scientific career should have come to an end in the State beloved by Franklin who first inspired Priestley's zeal for experimental philosophy.

In Joseph Priestley's "versatility, eagerness and humanity; in the range of his curiosity concerning all things physical, moral or social," we find an outstanding example of the intellectual energy of the eighteenth century that characterized the beginnings of our Society, and that has given us a tradition of vitality and breadth of scholarly interests.

BENJAMIN SMITH BARTON AS NATURALIST

FRANCIS W. PENNELL

Curator of Plants, Academy of Natural Sciences of Philadelphia

(Read February 13, 1942, in Symposium on the Early History of Science and Learning in America)

For twenty years now I have held an unofficial position in this Society, that of Curator of your botanical collections. In 1897, after Thomas Meehan's successful search in your attic for the plants gathered by Lewis and Clark, all such specimens—and they were mostly interesting and old—were deposited at the Academy of Natural Sciences of Philadelphia. Since then, most of these have been mounted and all (except for a few recently found) have been placed in modern steel storage cases. Sometime I want to tell you of all these treasures, and as a beginning, I welcome today's chance to bring to your attention one of them, the herbarium of Benjamin Smith Barton. It was this series of 1,674 sheets, to which we find there are yet more plants to be added, that first drew my interest to this early Philadelphia botanist.

The elder Barton, as we may call Benjamin Smith to distinguish him from his nephew and botanical successor at the University of Pennsylvania, William P. C. Barton, author of the first flora of the vicinity of Philadelphia, was more than a would-be writer of floras of Pennsylvania and Virginia, he was the actual author of the first general outline of botany written and published in the United States. He was more than a botanist, however, for his interests lay strongly in zoölogy as well, and in that field he held the earliest professorship in America, along with what was essentially the earliest in botany, too. His interests were wider than biology, even, for he was a medical professor as well as practitioner; and, furthermore, he was a pioneer in the study of our native Indians.

Benjamin Smith Barton was almost by birth a member of the American Philosophical Society. His father, Rev. Thomas Barton, whose interest in natural history made him a correspondent of Linnaeus and whom his son credits with likely the best collection of minerals in the Colonies, was a member of this Society, and to him belongs the credit of discovering the genius of David Rittenhouse. His mother, Esther, was the sister of David Rittenhouse, so that your second president was Benjamin Smith Barton's own uncle. Finally, his oldest brother, William Barton, was Benjamin's predecessor as a member of this Society, and in 1791 contributed to its *Transactions* a detailed statistical paper entitled "Observations on the Probabilities of the Duration of Human Life, and the Progress of Population, in the United States of America"; by profession a lawyer, William's main published work is his "Memoirs of the Life of David Rittenhouse." Through uncle, father, and brother, close, indeed, were Benjamin Smith Barton's ties to this Society.

Thomas Barton, the father, had attended Trinity College, Dublin, and then came to Pennsylvania in 1750, the following year becoming a tutor in the Academy of Philadelphia and also teaching a small school not far from the Rittenhouse home near Norristown northwest of this city. It was then that he was of most help to young Rittenhouse, and in 1753 he married David's older sister. But Thomas had decided on a clerical career and so returned to England for his ordination in the Church of England in 1755. Then followed frontier evangelical work with the Indians around Carlisle, Pennsylvania, before he became settled, with a church in Lancaster, where he and Esther established their home on the banks of the Conestoga Creek. This was Benjamin's boyhood home. Thomas Barton's career continued checkered—with friendship so keen for Provost William Smith of the College of Philadelphia as to insert a Smith in his son Benjamin's name, with honorary degrees both from the College of Philadelphia and from King's College, New York, he was repeatedly in difficulties with the Scotch-Irish frontier folk who looked with Presbyterian coolness on a clergyman of the Church of England direct from England; and later, when preceding the Revolution he would not violate his ordination vows by omitting the prescribed prayers for the King, they boarded shut his church. That was in 1773; the next year his wife Esther died, leaving eight children, mostly young; two years later

Thomas Barton married again, but it seemed expedient for him to leave Lancaster, and so it was that he died in 1780 in New York, where he was awaiting passage to the safe asylum of England. He protested devotion to this country, and that he could not in conscience do otherwise than he did; his children, not having his hurdle to cross, seem to have all grown up enthusiastic Americans.[1]

So it was that Benjamin Smith Barton, born in 1766, was but eight years old when his mother died, and only fourteen when he lost his father. He was one of the younger children in this large family. Six of the eight were boys, and at least three of his brothers were also interested in scientific matters. Of William I have already told. Matthias in later years made such drawings of the fishes of the Susquehanna River that Benjamin could say: "As the production of an untaught artist, who paints for his amusement, the drawings are uncommonly neat and even elegant," adding that "it is my intention to publish engravings from these drawings." Richard Peters Barton, who lived in the Shenandoah Valley, repeatedly sent in notes to Benjamin's *Medical and Physical Journal*. To both of these brothers Benjamin dedicated parts of this journal. But to return to the youth of these orphans.

Their father had provided that his "minor children" be placed in suitable care. Benjamin, as a boy, took to study rather than to "active bodily pursuits," and greedily absorbed knowledge. From fourteen to sixteen he was at an Academy in York, and emerged an "avid" reader of Greek and Roman authors. It was at sixteen, as we are further informed by his nephew William P. C. Barton,[2] that he "made his first attempt in composition," and—note the theme that appealed to this earnest contemplative youth—it was entitled an "Essay on the Vices of the Times."

Already the features were setting that were to mark Benjamin Smith Barton throughout life. There was an ardent temperament, that was not adequately supported physically, for he never became robust. There was breadth of interest, with strong liking for both history and natural history. There was rare skill in drawing, which in his mature scientific work was rigorously held in check by his passion for accuracy.[3] In spite of an early ultra-seriousness there must have been a fire about him that led to his becoming a keenly interesting personality.

Our time is too brief to follow his life in detail, but let us try to grasp its general course. From sixteen to twenty he was in Philadelphia, living at the home of his oldest brother, taking general studies at the College of Philadelphia and at eighteen commencing medical training there under the direction of Dr. William Shippen, an able teacher, the founder of the College's Medical School, and one of the early members of the American Philosophical Society.

Next comes another special experience, his selection, at the age of nineteen, to accompany his uncle David Rittenhouse on the commission to lay out the western boundary of Pennsylvania. He was gone from May to October, 1785. At the Historical Society of Pennsylvania there exist two manuscript books, in which Barton has later copied by topics rather than strictly chronologically his original journal, leaving after each entry sufficient space for later comment.[4] This redaction shows the keenness of his observations on geography and geology, on animals and plants, and especially shows the awakening of his interest

[1] "The Reverend Thomas Barton," by Rose G. Barton, and read by Frederick Marx Barton, in *Papers* Lancaster County Historical Society, **30**: 101–104, 1926; "Thomas Barton, a Pennsylvania Loyalist," in Ontario Historical Society *Papers and Records*, **30**: 33–42, 1934; William Barton's *Memorials of Rittenhouse*, p. 278–280, 1813. At the Historical Society of Pennsylvania there is a photostat copy of what is entitled a *Commonplace Book containing Rittenhouse Letters, etc.*, that had been loaned by Miss Fanny D. Abbot. This contains much unpublished matter, as well as excerpts from scattered works, concerning Thomas Barton and his descendants through his son William. It lists the eight children of Thomas and Esther Barton in what is obviously not a chronological sequence, since Benjamin S., born 1766, appears second; Matthias, born 1762, third; and Esther, born 1756, sixth. Placing the children two years apart from 1754, the presumed year of William's birth, and allowing for the likely naming for parents and grandparents before uncles and friends, leads me to suggest the following possible sequence: William, Esther (m. 1774 Paul Zantzinger), Thomas, Julianna Susanna (m. Thomas White), Mathias, Richard Peters, Benjamin Smith, and David Rittenhouse (m. 1793).

[2] *"A Biographical Sketch, read pursuant to Appointment before the Philadelphia Medical Society, at a stated Meeting on Saturday, 16th February 1816, of their late President Professor Barton."* 34 pages. Published separately.

[3] His nephew thought him unduly meticulous, when in drawing for his uncle a spiny lizard, Professor Barton insisted that each spine should be faithfully shown.

[4] Internal evidence, when describing Pittsburgh, gives us 1799 as the date of his redaction. Doubtless this was in preparation of "the View of my *Travels* through different parts of the United-States," an unfilled project to which he alludes in Note P on page 85 of his *Discourse on Some of the Principal Desiderata in Natural History* of 1807.

in the Indians, their monuments and their languages. As to his uncle a tribute has been subsequently inserted at the place where the young Barton tells of Rittenhouse's leaving the party on September 12 to hasten back to Philadelphia. But it was years later, in giving an account of the last sickness of his uncle, whom he had attended as physician, that B. S. Barton speaks with full feeling of all he owed to Rittenhouse:[5] "He was a father and supporter. He laid the foundation of what little prosperity in life I now, or may in future enjoy," and "It was David Rittenhouse who enabled me to be useful."

Next came two years of medical study in the University of Edinburgh, culminating in his winning the coveted Harveian Prize by a dissertation on the Black Henbane—the first of Barton's papers on medicinal plants, a large division of what is known as "Materia Medica." But before this achievement he had brought out in Edinburgh in 1787 part I of his *Observations on Some Parts of Natural History,* this dealing with such Indian mounds as he had seen in western Pennsylvania and suggesting that their builders might have been the Toltecas of Mexican antiquity; it was dedicated to his brother William Barton, but later we find Benjamin apologizing to his older brother for this premature work, and he never went further with the series.

From Edinburgh Benjamin went on to Germany for a year's study, and I suppose that his M.D. from the University of Göttingen must be one of the first degrees from that country conferred upon an American.* As to the journey back to the United States there has most happily survived at the Historical Society of Pennsylvania a small yellow book, being his journal of the crossing from Gravesend to Philadelphia from July to September, 1789, in the *Apollo,* William Billings, Captain. Entries on sharks, porpoises, flying fish, gulfweed, etc., show him keenly observant, while he takes down many historical and other notes from his elderly fellow-passenger, John Pemberton.

It was on his return to this country in 1789, at no more than 23 years of age, that Dr. Benjamin Smith Barton was made a professor in the College of Philadelphia, a position continued when two years later the College changed its name to the University of Pennsylvania. He was asked to teach a subject never before taught in America, Natural History, and with it another subject nearly as novel, Botany. (The only previous courses in the latter had been given in the same institution a few years earlier by Dr. Adam Kuhn.)

From 1789 until his death in 1815 Professor Barton's work lay in the University of Pennsylvania. In 1795 he succeeded to the professorship of Materia Medica, and for most of his active life subscribed himself as "Professor of Materia Medica, Natural History and Botany." Finally, in 1813, and so only two years before his death, he succeeded Dr. Benjamin Rush as "Professor of the Practice of Physic," retaining with it his original chair of Natural History and Botany. His nephew tells us truly, that Professor Barton was "ambitious." He must have been an able and inspiring teacher as well.

It was also in 1789, but before his return from Europe, that Benjamin Smith Barton became a member of the American Philosophical Society. From 1790 to 1800 he was one of the Society's Curators. In 1802 he became one of the Vice-Presidents, a position held until his death. Until 1804 a series of papers from his pen appeared in the *Transactions* of this Society, but from that year *The Philadelphia Medical and Physical Journal,* of which he was founder and editor, became his habitual medium for shorter papers.

From 1790 Dr. Barton was a Fellow of the newly founded College of Physicians, and from 1809 until his death in 1815 he was President of the Philadelphia Medical Society. From 1798 he was one of the physicians connected with the Pennsylvania Hospital.

Doubtless from 1789 Dr. Barton must be thought of as a medical practioner, but his nephew tells us that his practice was "never very extensive,"[6] and speaks of him as much better acquainted with the theoretical than the practical side of medicine. Quick in his diagnoses, he was

[5] William Barton's *Memoirs of Rittenhouse,* p. 441–445, 1813.

* In his address on "Benjamin Smith Barton," delivered at the University of Pennsylvania in 1916 and published separately (27 pages), Dr. Edgar F. Smith tells of the vain search made by Dr. D. B. Shumway through the rolls of the University of Göttingen for any mention of Benjamin Smith Barton. We might suppose that the actual degree came from Edinburgh, were it not for Benjamin's clear statement in his letters to his brother William that he received it at Göttingen. But the diploma is lacking from the series of ten honorary certificates from American and European learned societies that is preserved at the Historical Society of Pennsylvania. Dr. Smith's address was reprinted in *Papers Lancaster County Historical Society,* **28**: 59–66, 1924.

[6] A day-book, that has been found at "Montgomery Place," shows a fairly busy practice.

extremely cautious in his doses. That he was "soothing in his behaviour to his patients" might to some degree result from the sympathy resulting from personal troubles, for Dr. Barton himself had—to quote his nephew—as "his continual earthly companion—irregular gout." That "he was irritable and even cholerick," with "repeated vacillations between equanimity and depression," was due to its attacks.

All his life long there remained this menace of poor health. His enthusiasm was constantly meeting handicaps, so that achievement was repeatedly curtailed. As we glance at Professor Barton's works, we are conscious of a remarkable number of endeavours, commenced but not fulfilled. I would not lay all the responsibility for this upon Barton's poor health, for we know of the marvellous achievements of a Darwin under like conditions. There was doubtless also something temperamental, that led Barton to see the possibilities of subject after subject, each soon abandoned with the tedium of effort. I suspect that this course is one into which a person of Barton's semi-invalidism very readily drifts.

In a few words his nephew brings before us the appearance of the elder Barton. "In figure he was tall, and exceedingly well formed. . . . His [face] was strongly expressive of intelligence, and his eye was remarkably fine and penetrating." "But few men made, at first sight, a more vivid and favorable impression," is the memory of his student, Charles Caldwell. Also, he was an excellent conversationalist.

In contrast to that detachment usual to modern scholarship, Barton speaks personally with his readers. He regrets the bonds of occupation and health that prevent him from accomplishing more, and habitually informs us that the present work is but the prelude to what he has in preparation upon the subject, often giving the title of the greater work anticipated. You are in the author's confidence, as to his dreams and hopes. Perhaps this personal touch is the key to Barton's success with his students.[7] His earnestness and enthusiasm are ever present.

So also are his ideals. Toward the end of the "Discourse" about to be mentioned, are several significant paragraphs discussing how injurious is credulity to the character of the naturalist or the historian. And with regard to keeping an open mind in experimental work, we have this written in 1794: "In conducting a series of experiments it is ever a matter of importance that the experimenter should be free from the dominion of prejudice and system. Perhaps, facts are never truly related in all their unadulterated purity except by those, who, intent upon the discovery of truth, keep system at a distance, regardless of its claims. The strong democracy of facts should ever exert its wholesome sway."[8]

Nowhere can one so well appreciate the breadth and harmonious proportions of Barton's scientific interests as in the address that he delivered in 1807 before the Philadelphia Linnean Society of which he had been elected the first president.[9] It was entitled "A Discourse on Some of the Principal Desiderata in Natural History, and on the best Means of promoting the Study of this Science, in the United-States." He presented these under several heads. In Zoölogy he includes the physical history of Man, and so urges exploration of Indian mounds and the assembling of Indian vocabularies so as to judge of the origins and relationships of these peoples; then (still under Zoology) he suggests observations of habits of mammals; advocates the study of birds in relation to migration and possible dormancy; of Amphibia, with which he includes Reptiles, as to kinds and habits; of the neglected field of Ichthyology; of the practical and vast field of Entomology; and of the complex and little-known field of Helminthology. Then, under Botany in which he considers knowledge more advanced than in Zoölogy, he discusses the need for monographs of various genera of flowering plants, for attention to Cryptogamic plants, for geographical studies and comparisons, and for observation, especially through information to be gathered from the Indians, as to what native plants may contribute to our medical and food resources. Under Geology and Mineralogy he notes how little has yet been done to-

[7] Concerning his teaching, Dr. William S. Middleton in his account of Benjamin Smith Barton published in 1936 in the *Annals of Medical History* (n.s., **8**: 477–491) says of him that "even his hypercritical student Charles Caldwell was forced to admit that as a teacher of botany he possessed at least one very high and important quality—an earnest and exciting enthusiasm, by which he induced his pupils to engage in the study of the sciences with a corresponding earnestness, accompanied by a resolution to teach themselves."

[8] "A memoir concerning the fascinating faculty which has been ascribed to the rattle-snake, and other American serpents." *Trans. Amer. Philos. Soc.*, **4**: 91.

[9] How long the Philadelphia Linnean Society lasted I do not know, but that it survived Barton's life-time is shown by a certificate of membership dated 1816 that is preserved at the College of Physicians of Philadelphia; at that time William P. C. Barton was president.

ward a knowledge of the distribution of strata with their attendant fossil plants and animals, and suggests observations on the occurrence of special minerals, etc. Under Meteorology he suggests assembling records as to possible changes of climate since the settlement of our colonies. Everywhere he points to special problems awaiting investigation.

Broad as this "Discourse" is, few are the fields of interest suggested that Barton has not touched upon himself in some of his varied publications. Accordingly, we may follow the above framework in making a brief survey of his published works. To it should be added yet another field, that of Medicine. But we shall leave for the last the consideration of Botany.

Indian antiquities and philology remained throughout life a keen interest. He was ever gleaning information, and there survives in your Library a list of 23 questions, drawn up in March, 1797, with answers concerning certain tribes from D. Zeisberger and John Heckewelder; these questions deal with physical and medicinal matters, and are accompanied by a request for words in certain Indian languages. Barton published brief papers on articles that were taken from Indian mounds in Ohio and elsewhere, as well as comparative studies of words. His most considerable work in this field was entitled: *New Views of the Origin of the Tribes and Nations of America,* a book issued in Philadelphia in 1798, and dedicated to Thomas Jefferson. The body of this work consists of tables of familiar words in many Indian languages, and in those of certain tribes of Asia. There is a "Preliminary Discourse" of more than 100 pages, wherein many conclusions are reached, of which the most fundamental is that "The comparative vocabularies which are published in this memoir, seem to render it certain, that the nations of America and those of Asia had a common origin." Also he thinks that the linguistic evidence contradicts Jefferson's opinion that there are more languages among the Indians in America than among the peoples of Asia, and so that the nations of America are of the greater antiquity; reversing this evidence, but holding the reasoning, Barton thinks that the Indians came hither from Asia. And, pertinent to certain recent discussion, he suspects that "many hundred, perhaps three or four thousand, years have been necessary to produce the difference of dialects which we observe between many American and Asiatic nations."

Of strictly zoölogical papers Barton has many. Mammals were described as new to science, as in the account of a *Dipus,* or jerboa, that appeared in 1795 in your *Transactions;*[10] or their relationship and occurrence were discussed, as in his papers on native dogs,[11] on the American elk,[12] and that in which he mistakenly thought that the Rocky Mountain sheep was identical with the taye of Asia.[13] Of a different character was his study, entitled: *Facts, Observations, and Conjectures relative to the Generation of the Opossum of North America,* published separately in 1809.

Birds figure in Barton's *Fragments of the Natural History of Pennsylvania,* of which Part I was published in Philadelphia in 1799. In this he considers bird migrations, presenting tabular records of arrival that are correlated with temperature and the progress of vegetation; and he considers also our bird residents. As to further Parts of the "Fragments," he reports two others ready to follow, "if this is favorably received"; but they were never issued.

Reptiles early received consideration in two papers on the rattlesnake. In 1791 there appeared in your *Transactions* "An Account of the most Effectual Means of preventing the Deleterious Consequences of the Bite of the Crotalus horridus, or Rattle-Snake";[14] and in 1794 "A Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-Snake, and other American Serpents."[15] The latter shows remarkable knowledge of such claims from antiquity to the present, but interprets the supposed instances of birds being so attracted as due to parental desire to draw off the snake from their nests, or of squirrels as being cut off from their holes. In 1803 Barton announced a forthcoming work on "The Anatomy and Physiology of the Rattle-Snake, and other North American Serpents," but of it we have only the elaborate colored drawings that have survived at "Montgomery Place."

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[10] "Some account of an American species of Dipus, or jerboa." *Trans. Amer. Philos. Soc.,* **4**: 114–124.

[11] "Some account of the different species and varieties of native American, or Indian dogs." *Phila. Med. & Physic. Journ.,* **1**, pt. *2*: 3–31, 1805.

[12] "Account of the Cervus Wapiti, or southern elk of North America," in *Suppl. Phila. Med. & Physic. Journ.,* 36–55, 1806.

[13] "Some account of the taye, a species of sheep," *Phila. Med. & Physic. Journ.,* **2**: 106–113, 1805.

[14] *Trans. Amer. Philos. Soc.,* **3**: 100–115.

[15] *Trans. Amer. Philos. Soc.,* **4**: 74–113. Also reprinted separately, with a last edition in 1814.

Further studies of reptiles include in 1803 the description of a new lizard from the vicinity of Philadelphia,[16] and in 1805 a discussion of torpidity in the North American alligator.[17]

At least two studies were published on what we include in Amphibia, one in 1808 on *Siren lacertina,*[18] and the other in 1812 on that curious animal of our interior country called the hell-bender.[19]

Barton seems to have published only a few trivial notes about fishes.

Nor did he publish much about insects. In 1793, however, there is "An Inquiry into the Question whether the *Apis mellifica,* or true Honey-Bee, is a Native of America," published in the Philosophical Society's *Transactions.* After reviewing both Spanish- and Anglo-American records of observation, he is confident that this insect was early introduced from Europe. Indian tradition was emphatic as to this, as it was also about such a widespread plant-pest as *Plantago major,* to which the Indians gave the name of "Englishman's Foot." Our common woolly mullein he puts confidently in this category. Indeed, his consideration of this matter of early naturalization from Europe is throughout pertinent. In this paper, as indeed in much of his work, he places implicit reliance on William Bartram's accuracy and judgment; "A man of rigid veracity," he elsewhere calls Bartram. A further paper in the *Transactions* for 1802 gives "Some Account of the Poisonous and Injurious Honey of North America," but that is more botanical than entomological, since it is concerned with ascertaining from what plants the bees gathered such honey.

But Barton planned more than this as his contribution to the knowledge of insects, for there is the record of the meeting of the American Philosophical Society for December 21, 1804, at which it was announced that the Magellanic Premium, awarded annually for the most meritorious paper "relating to Navigation or to Natural Philosophy," had been conferred upon the author of one dealing with a "number of the pernicious Insects of the United States." Upon opening a sealed letter, it was discovered that the author was Benjamin Smith Barton. In spite of such recognition I suppose that Dr. Barton came to realize that he was here on unfamiliar ground, for the work never reached publication, and in his final memoranda concerning the disposition of his papers he directed that it "be *not* presented to the Philosophical Society or to the Public."

Helminthology is a field in which Dr. Barton seems never to have worked.

There remains Palaeontology, so largely pertaining to both zoölogy and geology. Large fossil bones intrigued Barton as they did Jefferson, and so we find him in 1805 publishing "Facts, Observations, and Conjectures, relative to the Elephantine Bones (of different species) that are found in various parts of North America,"[20] this characteristically taking the form of a letter to Cuvier, the great French zoölogist. In 1814, the year before Barton's death, there appeared the beginning of what was to have been a considerable work in this field, the first part of his "Archaeologiae Americanae Telluris Collectanea et Specimina; or Collections, with Specimens, for a Series of Memoirs on certain extinct Animals and Vegetables of North America; together with Facts and Conjectures relative to the ancient Condition of the Lands and Waters of the Continent." Sufficiently comprehensive in scope!

In the fields of Medicine and the physical study of Man Barton published many papers. Among these is one on goitre;[21] another concerning an albino negro, with general consideration of albinism;[22] and various shorter papers and notes in the *Philadelphia Medical and Physical Journal.*

Materia Medica deals mostly with plant products, so that it is a natural sequence to pass by this route from medicine to botany. I suspect that his *Collections for an Essay toward a Materia Medica of the United States,* originally published in Philadelphia in 1798 as a book of 49 pages, but reaching a third edition of 120 pages in 1810, has

[16] *Trans. Amer. Philos. Soc.,* **6**: 108–112.

[17] *Phila. Med. & Physic. Journ.,* **1**, pt. 2: 106–109, 1805.

[18] "Some account of the *Siren Lacertina,* and other species of the same genus of amphibious animals in a letter to Mr. John Gottlob Schneider of Saxony, with an outline engraving of the animal, from a finished drawing made by myself."

[19] "A memoir concerning an animal of the class of Reptilia, or Amphibia, which is known in the United States by the name of alligator and hell-bender, with an engraving."

[20] *Suppl. Phila. Med. & Physic. Journ.,* 22–35.

[21] *A memoir concerning the disease of goitre, as it prevails in different parts of North America.* Philadelphia, 1800. (93 pages.)

[22] "Account of Henry Moss, a white negro, together with reflections on the affection called by physiologists *Leucaethopia humana;* facts and conjectures relative to the white colour of animals, and observations on the colour of the human species." *Phila. Med. & Physic. Journ.,* **2**, pt. 2: 3–18, 1806.

real value as an historic landmark in its field. It classifies plants into ten series according to their active properties as "astringents, tonics," etc., and for each plant we are told what parts should be used and how they should be prepared and applied; what gives the work real value is that the plants are all native American species. It is for his work in this branch of medicine that Dr. Barton is still held in memory by the medical profession of America, and Dr. Middleton's recent sketch, already quoted, calls him the "Father of American Materia Medica."

The food properties of plants likewise interested Barton, and led to some notes and papers. Probably the most considerable of these is his account of the native home of the potato, that appeared in *Nicholson's Magazine*. In the whole field of the uses of plants by Man, Barton was keenly interested, and his purely botanical works abound with popular lore as well as scientific comment.

Botany is undoubtedly the field that Barton cultivated most successfully, but even here his achievements were far exceeded by his plans. He was interested in experimental work, as is shown in such short papers as that on the stimulant effect of camphor on plants.[23] This interest is well seen in the physiological part of his *Elements of Botany, or Outlines of the Natural History of Vegetables*, of which the first edition appeared in Philadelphia in 1803 and the second in 1812–14.

The *Elements of Botany* will remain Barton's most important work. It considered botany broadly. For its illustrations he used not only his own skill in drawing, but also that of Pierre Turpin and of William Bartram. Although planned and functioning as a textbook, it is actually more a critique of current opinions than a work of reference. From the vantage-ground of our later knowledge it is interesting to read Barton's surmises as to the function of the leaves, and his rejection of the new idea of the German Sprengel that the corolla of flowers is for the convenience of the visiting and pollinating insect; the idea evidently seemed fantastic, and so he favored the view that the corolla must have to do with the physiological process of respiration. The secretion of nectar is a correlated problem, concerning which, after duly considering the new claims that it is for the attraction of insects, these were rejected in favor of its serving as a source of nourishment to the developing stamens and pistils. Yet Barton was abundantly alive to the need of pollen passing from anthers to stigmas, only he considered that this regularly happens within the same flower, an habitual self-pollination; only in the case of dioecious plants, where the sexes are obviously separated over a space of ground, and in that of *Sarracenia* where the expanded umbrella-like stigma seemed too grand an obstacle, does he grudgingly consider that insects may assist.[24] But perhaps his skepticism here is part of his general caution, for again he calls for solid factual construction in advance of our theories. After quoting John Hunter that "Life is a property we do not understand," he adds: "And we never shall understand it, if we attempt to construct systems, before we know how or where to collect facts" (1: 280).

The systematic part of the *Elements* follows methodically through Linnaeus' classes and orders, but on inspection one finds that Barton's treatment is largely a criticism of this outline with suggestions for its improvement that involve much leaning toward Jussieu's coming scheme of plant-families. Indeed, while rendering all homage to the immense service that Linnaeus had done botany, Barton says: "The Sexual System of Linnaeus cannot be immortal. It will, at some future period, be deserted for a system more agreeable to the scheme or intentions, of nature" (1: 144).

Quotations, especially from Erasmus Darwin and from classical authors, appear abundantly. And, now and then, you get an opinion on the issues of Barton's own time, as in this fervid outbreak when considering the rice plant. "But how greatly it is to be regretted, that this vegetable, in the most free and happy country upon earth, should be cultivated, almost exclusively, by the hands of slaves! Shall we never learn to be just to our fellow creatures? Shall we blindly pursue the imaginary advantages of the moment, and neglect the still but solemn voice of God, until

> 'Vengeance in the lurid air,
> Lifts her red arm, expos'd and bare.' "
> (3: 30)

This, in 1803!

[23] "Hints relative to the stimulant effects of camphor upon vegetables." *Trans. Amer. Philos. Soc.*, **4**: 232–235, 1796.

[24] But he failed to understand the purpose of small insects in visiting the flowers of our common milkweed, *Asclepias syriaca*, and, after observing what he took to be house-flies (presumably other flies or small bees) getting hopelessly caught in the flowers, he thought that he had discovered a new insectivorous plant, and so read his "Memorandum concerning a new vegetable muscipula" to this Society in 1803, a paper that was published in the Society's *Transactions*, **6**: 79–89.

Turning to Barton's interest in the systematic side of botany, I wish first to mention three papers in the *Philadelphia Medical and Physical Journal* that tell of some of the earliest collectors of our eastern flora. In 1805 he presented some "Memorandums of the Life and Writings of Mr. John Clayton, the celebrated Botanist of Virginia."[25] Barton greatly admired Clayton, senior by some years to John Bartram, and whose plants, forming the basis of Gronovius' *Flora Virginica,* contributed more than any others to Linnaeus' knowledge of our flora. How characteristically Barton closed his sketch: "It is my intention to give a more ample and finished account of the life and writings of Clayton, in another work, exclusively consecrated to the memory of a number of illustrious men, botanists, naturalists, physicians, and philosophers, who were either born, or who have flourished, in North-America."

In the other two sketches he went farther back to the botanists of the seventeenth century, who were sending plants to England before John Bartram was born. These both appeared in 1806, and were entitled: "Some Account of Mr. John Banister, the Naturalist," and "Memorandums of the Lives and Literary Labours of Mr. William Vernon and Dr. David Krieg."[26] We owe a real debt to Barton for perpetuating some knowledge of these true pioneers, whose specimens contributed much to the great pre-Linnaean works of Plukenet and Ray. Banister collected in Virginia, and Vernon and Krieg in Maryland.

In considering Barton's own efforts in floristic and taxonomic botany it is necessary to realize the extent of his travels and also what help he received from correspondents and students.

We have seen that Barton as a youth was one of Rittenhouse's party in 1785 to survey the western boundary of Pennsylvania, but he seems to have made no collections of plants nor did he subsequently publish notes upon the flora seen.

His earliest systematic paper was one read to the Philosophical Society May 28, 1792, and entitled "A Botanical Description of the *Podophyllum Diphyllum* of Linnaeus."[27] Realizing how different is the twin-leaf from the may-apple, *Podophyllum peltatum,* Barton proposed the former as a new genus *Jeffersonia,* illustrating his account with drawings by William Bartram and himself.[28] The paper was both descriptive and historical, and takes Barton's habitual form of a letter, this time to Dr. Thunberg, who was Linnaeus' successor as professor at Upsala, Sweden. Evidently this was because he wished to draw Thunberg's attention to certain of his critical comments on the Linnaean Sexual System. As to the origin of *Jeffersonia,* Barton wonders if it may have arisen as a hybrid, at least it is "a beautiful example of a connecting medium between *Podophyllum* and *Sanguinaria,*" our may-apple and bloodroot. An historical touch is his assertion that to Clayton rather than Gronovius should belong the credit for the merit of the *Flora Virginica.*

Barton's herbarium presumably started with the two booklike volumes of specimens in your series at the Academy of Natural Sciences. These contain some plants of his own, all from near-by localities, as Bartram's Garden, etc., but most of these plants were gathered by other persons, especially by Major Adlum from upstate Pennsylvania and by André Michaux from various parts of eastern North America. From 1797 on specimens were left unmounted. That was the year of Barton's longest American journey—already one taken for his health. It would not be counted extensive to-day, for it was only across New Jersey to New York, up the Hudson River to Albany, and across New York State to Niagara Falls. His journal of this trip, in its present amplified form spaced for later insertions just as was the earlier one of 1785 from western Pennsylvania, has survived in two manuscript books, the one carrying the journey as far as Old Fort Schuyler being now at "Montgomery Place," the other which takes Barton on to Niagara being at the Historical Society of Pennsylvania. Both are primarily filled with observations and notes about Indians. It is tempting to follow Barton through this course, but as it produced no special botanical report, I have been content with showing in our exhibit one of the record-books and a few of the plants gathered.

For the next decade Barton's herbarium shows accessions. He himself added specimens from Philadelphia and vicinity with dates of 1797, 1798, 1799, and 1803. In 1802 we have his plants from a trip to the Shenandoah country of Virginia, and on to Staunton and beyond; in 1803 from Baltimore; in 1805 from another trip to the Shenan-

[25] Vol. 2, pt. 1: 139–145.

[26] Vol. 2, part 2, pages 134–139, and 139–143, respectively.

[27] *Trans. Amer. Philos. Soc.,* **3**: 334–348.

[28] The plant is shown in to-day's exhibition from a later painting by Turpin that was evidently prepared for Barton's *Prodromus.*

doah, with some from Baltimore, Hagerstown, and Washington en route; and in 1806 from Passaic Falls, New Jersey.[29] But all of Barton's specimens together can scarcely total 200, nor do they show evidence of much study by him. It was afield, with note-books rather than with drying papers, that Barton worked. We must speak of two of his projects.

In 1799, in the first and only published part of his *Fragments of the Natural History of Pennsylvania,* Barton spoke of another and "more extensive work on the Vegetables of Pennsylvania and some of the adjoining States" that was to appear "I do not know when. For who does not know that the promises of authors are like the promises of lovers?" Evidently this was a favorite project, due to receive loving care. Finally, after a more definite notice in 1805 that announced the title-to-be, there appeared in the First Supplement to the *Philadelphia Medical and Physical Journal* for March, 1806, a preliminary sample of his *Prodromus of a Flora of the States of New-York, New-Jersey,* PENNSYLVANIA, *Delaware, Maryland, and Virginia.*[30] Mrs. Violetta W. Delafield's search among the Barton papers at "Montgomery Place" has produced evidence that in the preceding year, 1805, 500 copies of this *Prodromus* had been printed. Yet no copy of the work is known to exist, and we can show you now only this sample and a few scorched proofsheets from "Montgomery Place."

My first knowledge of the Pennsylvania *Prodromus* came through the quotations from it in Barton's later *Flora Virginica;* as these records gave exact pagination, I could compare the two and so ascertain that at least two fascicles of the former had been printed, and I suggested that each might have been of 96 pages.[31] At that time I was so impressed with the fullness of Barton's records and field-observations as to say, were such to be found, "I think that the 'Prodromus Florae Pennsylvanicae' would prove significant because of its information of the flowering and fruiting seasons, the duration, the habitats, the history and the uses of our plants, and would be found to contain colloquial names beyond any work that has been produced up to the present day." From this sample the last proves true enough, names used by the Indians as well as those by the settlers being freely given; indeed, my supposition is in the main justified, although there is more synonymy and discussion of general distribution than anticipated. Barton gleaned widely from literature as well as from nature. The work was to have been illustrated, the author telling us that "many of the drawings are already prepared," and that some of these were by the French artist Turpin. (Those that have survived show high artistic quality.)

As usual, this *Prodromus* was merely a beginning, but what became of the copies of it that were printed? They certainly survived until the preparation of the *Flora Virginica* that was issued in 1812, but the work was not mentioned by its author in his final memoranda of November, 1815, even though these deal with just such material. Mrs. Delafield has sought in vain for mention of this *Prodromus* among Barton's papers after 1812. Evidence would seem to point to its destruction by Barton himself. But why should he destroy a work on which he had lavished such special care?

We do not know the answer, but I suspect that the coming of a remarkable taxonomic assistant revealed to the sensitive professor what unsuspected flaws there were in his *Prodromus.* Reviewing our sample of it in the light of later knowledge, one notices the wrong assignment of Crantz' description of a Greenland plant to *Mitchella repens;* and that the southern *Callicarpa americana* (not given in any modern manual north of Virginia) is attributed to New Jersey, Maryland, Delaware, and questionably to Pennsylvania. Just after the publication of this sample, Barton took into his employ the young German immigrant, Frederick Pursh, who had been in charge of the unexcelled garden that William Hamilton had developed at his estate, "Woodlands,"[32] along the Schuylkill River above the old Bartram's Garden. He financed Pursh on a pedestrian tour that passed by Carlisle, Pennsylvania, and Hagerstown, Maryland, across the Potomac River at Harpers Ferry to the Shenandoah Valley, whence Pursh spent the summer of

[29] Barton had visited these falls years earlier as shown by an entry in his journal of 1797 while at the Falls of the Mohawk River near Cohoes, New York, alluding to a visit to "the Falls of Passaeg, in Jersey, in the year 1791?"

[30] Pages 77–92.

[31] "The elder Barton—his plant-collection and the mystery of his floras." *Bartonia,* **9**: 17–34, 1926. Comparative table on page 32. The pagination of the second or of a later fascicle exceeded my calculation, since the proofs at "Montgomery Place" are paged from 115 to 122.

[32] The present Woodland Cemetery in West Philadelphia.

1806 tramping through the mountains of Virginia south eventually to Rocky Mount in Franklin County, and thence southeastward to Northampton County, North Carolina, after which in early autumn he passed northeastward to Norfolk and Accomac County in Virginia, and so home.[33] All but the last stages of this route can be traced with accuracy from the specimens that Pursh gathered, and which are in the Barton herbarium. These specimens were labeled by him with excellent statements of locality, a geographical precision remarkable for those days, and they numbered over 900. Barton, the busy professor, had never gathered his information so precisely, nor in such wholesale fashion.

Again in 1807 Pursh went afield for Barton, his pedestrian journey this year being to central New York and across to southern Vermont.[34] But the specimens in the Barton Herbarium from this second journey were barely 150. Beside those of these summers afield, Pursh has also about 120 specimens gathered at various places not far from Philadelphia. Altogether he contributed nearly 1,200 specimens to the Barton Herbarium, about six times as many as those gathered by Barton himself.

Beside those gathered by Pursh and Barton, there are nearly 400 specimens from other collectors in the Barton herbarium, all of which were likely assembled in this same decade, 1797 to 1807. Nearly thirty persons are represented in all, but some with very few contributions. Some were Barton's pupils, and some were men with whom he or Pursh made contact. Likely several owed their first botanical awakening to Barton, and in at least one another beside Pursh it meant the beginning of a productive botanical career. As Dr. John H. Barnhart has given an account of all these collectors in *Bartonia*,[35] I shall mention now only those who contributed at least 20 plants to Barton's herbarium. The largest series is that of 71 specimens from Southampton, southern Virginia, by a Dr. Gray, whom Pursh is known to have visited and for whose activity I suspect him responsible; next is that of 69 specimens from Aloysius Enslen, an Austrian plant-collector who travelled through the Southern States, and whose good specimens habitually lack data; next 58 specimens from David Thomas, a famous engineer, all of them gathered in the mountains near the West Branch of the Susquehanna River in north-central Pennsylvania; next 41 specimens from Peter James Beasley, M.D. 1807 of the University of Pennsylvania, which were obtained in Amelia County and perhaps elsewhere in Virginia; next 25 specimens from Augustus Shryock, possibly another of Barton's pupils, plants gathered near Chambersburg, Pennsylvania; and lastly 20 specimens from Thomas Horsfield, who received his M.D. from the University of Pennsylvania in 1798. Presumably Horsfield's few plants were gathered in Pennsylvania before 1801, as in that year he went finally to Java, which became the scene of his important botanical labors.

Although some of these specimens were labeled by the collectors, this task was often done by Barton, and yet more frequently by Pursh. The last, more often than Barton, supplied the identification, and this for plants of all collectors, Barton included. As I have said in another place: "Everywhere one notes that Pursh is more particular than his patron about precision of data. The internal evidence of the Barton Herbarium assures us that Pursh was the systematist responsible for the careful naming of the collection, and leads us to suspect that Barton, the professor, took only a moderate interest in strictly systematic botany and was depending upon his able helper to carry out the large plan portrayed in a statement given out in June, 1807." In his inaugural address to the Philadelphia Linnean Society, to which we have already alluded,[36] Barton says: "My own American Herbarium contains several hundred species of North-American plants more than are contained in the whole Flora of Mr. Michaux. Through the kindness of my friends and pupils, I am continually receiving accessions to the list. The history of my whole collection is intended for the public. Indeed, I have already made considerable progress in describing our plants."

[33] Pursh's journal of this trip is said to be among the Barton papers at the Historical Society of Pennsylvania, but I have as yet failed to identify it.

[34] Pursh's journal of this trip, in the possession of the American Philosophical Society, and entitled "Journal of a Botanical Excursion in the Northeastern Parts of the States of Pennsylvania and New York," was published in Philadelphia in 1866, and reprinted by the Onondaga Historical Association in 1923. Note that of Pursh's two trips for Barton each occurred one year later than the dates given in the preface of his *Flora Americae Septentrionalis*.

[35] "Brief sketches of some collectors of specimens in the Barton herbarium." *Bartonia*, **9**: 35–42, 1926.

[36] *A discourse on some of the principal desiderata in natural history*, page 40.

This year, 1807, signalized by such an optimistic statement, appears to be the last in which the Barton Herbarium received any accessions. Certainly few, if any, specimens were added later than this. That Barton failed to go forward with the amassing of specimens calls for an explanation; and, beside the continuous factor of increasing ill health, I think there is a very obvious reason why 1807 marked the termination of his herbarium's growth.

We have seen that Frederick Pursh had become the actual developer of the Barton Herbarium. Now Pursh spent the summer and early autumn of 1807 travelling in the northern states, and apparently in the late autumn or early winter moved from Philadelphia to New York in order to take charge of the Botanic Garden recently established there by Dr. David Hosack and which had been acquired by the state. Pursh remained three years in New York, then took a voyage to the West Indies, and shortly after his return therefrom went to England. There, with the generous patronage of A. B. Lambert, he was able to carry out an independent project of a flora of North America; Pursh's *Flora Americae Septentrionalis,* issued in 1814, marked a distinct advance on the earlier flora of Michaux.[37]

But the accomplishment of Pursh had been Barton's in inception, for the latter's 1807 speech and the former's preface in 1814 both lead one to the supposition that Barton, counting upon Pursh's help, was dreaming of a large American Flora. One wonders on what terms patron and assistant parted in 1807. Perhaps Pursh was motivated by a natural desire for independence, and thought that there was more of a future in the New York opening than ever developed. Or perhaps he wished to escape from thralldom to Barton, and may have suffered from his patron's irritability and even jealousy; one suspects the latter, since Barton's speech of 1807 gives no credit whatever to his brilliant helper. However it be, they parted, and Pursh eventually carried to England, along with some specimens of Lewis and Clark and perhaps of his second trip for Barton,[38] merely notes and descriptions from his large Virginia collection. These became the bases for certain new species in his *Flora Americae Septentrionalis,* and so the actual types are to be found, often quite unnamed, in the Barton herbarium.

It was about the time of Pursh's departure from Philadelphia that there came to the city a young English printer, Thomas Nuttall. Probably it was in 1808 that he made Barton's acquaintance, and the professor must have rejoiced at the appearance of so keen a recruit, just after the loss that he had sustained. In another place I have told of Nuttall's career, and of the very definite help that he received from Barton.[39] The earliest proven intercourse of the two was in 1809, when in June Nuttall wrote Barton several letters from southern Delaware, and sent him a bundle of plants that still survives among the deposits of your Society at the Academy.[40] It was marked by Barton: "Plants coll. by Mr. Nuttall, Delaware," and apparently was not added to his previous herbarium. In August and October of 1809 there were further letters from Nuttall, who had gone on another pedestrian tour, this time to Niagara and beyond to Ancaster at the western end of Lake Ontario. Then in 1810 began Nuttall's great travels, which the younger Barton long ago told us were made at his uncle's expense, and about which the recent finds at "Montgomery House" have given precise details. These are welcome since Nuttall, unlike Pursh, never publicly revealed his indebtedness to Professor Barton. We now have the Terms of Agreement, dated April 7, 1810, and signed by both Barton and Nuttall; Barton's "Directions for Mr. Thomas Nuttall"; and even a list of the articles that Barton lent the young man for the journey. (All these are reproduced in the Appendix to my account in *Bartonia.*) The "Directions" call for observations on Indians,[41] animals, plants, and minerals. Nuttall was to travel northwestward, through the Great Lakes country, including a course from Detroit to Chicago where "it is likely

[37] *Bartonia,* **9**: 24–25, 1926.

[38] These specimens passed into Lambert's herbarium, for which they were mounted on uniform small sheets of paper and the source inscribed by Lambert on the reverse of an upper corner. After that botanist's death in 1842 his collections were sold at auction and widely scattered, but the North American portion, including these plants taken by Pursh from series that otherwise passed to the American Philosophical Society, was purchased and presented by some unknown donor to the Academy of Natural Sciences of Philadelphia.

[39] "Travels and scientific collections of Thomas Nuttall." *Bartonia,* **18**: 1–51, 1936. For correction as to Nuttall's parentage and as to his English background, see "An English obituary account of Thomas Nuttall," in *Bartonia,* **19**: 50–53, 1938. For documents concerning Barton in relation to Nuttall see pages 45 to 51 of the first sketch.

[40] About these plants Dr. Robert R. Tatnall has published a special report, "Nuttall's plant collections in southern Delaware," in *Bartonia,* **20**: 1–6, 1940.

[41] Nuttall was supplied with numbered queries about Indians, presumably a copy of those preserved at the American Philosophical Society. He was especially asked about goitre among them.

that you will stand in need of a[n Indian] guide," from Lake Superior to Lake Winnipeg, and on to the Beaver River in 55° N.; he was to winter at one of "the British factories"; then, in returning, he was to strike southeast to the Missouri River, thence east to the upper Mississippi River, and, descending it, to return homeward via Fort Vincennes. What actually happened was that Nuttall reached Michilimackinak by going through Lake Huron, was picked up there by the Astoria Expedition and so wintered at St. Louis; in 1811 he ascended the Missouri River with the Expedition to the present North Dakota, returning alone in the autumn; and in the winter of 1811–12 descended the Mississippi to New Orleans, and there, finding what was brewing, made his way safely back to England before the outbreak of the War of 1812. He did, however, send Barton from New Orleans specimens, seeds, and a manuscript book, as acknowledged by Barton in his paper on "Bartonia," to be told of later. Nuttall, being English, could not return to this country before the spring of 1815, and it was likely that he reached Philadelphia in the autumn, after a summer in the Southern States. Barton, the American, had likewise taken advantage of the freedom of the seas to go to Europe from the spring to the autumn of 1815, so that he did not arrive back in Philadelphia until near the end of November or early in December. If the two met again, it must have been in the last days of Barton's life, since the invalid died on December 19. Of this later.

Barton's assistance to Nuttall on such a large program of exploration was likely in the hope of furthering anew his project of a Flora of North America, but that hope was nullified by the separation of patron and assistant caused by the War of 1812. Barton's health would not allow of such delay. Perhaps he dreamed of their working together, but there is no evidence that Nuttall ever saw or made any annotation in the Herbarium that had been studied by Pursh. But if the large project must be relinquished, there remained one of smaller compass, that was near to the Professor's heart. Over the years his favorite trips had been to the Shenandoah Valley of Virginia, where resided Richard and I suspect others of his brothers. There he doubtless went for rest from his strenuous teaching and for the fresh stimulus of observations afield. His herbarium shows plants gathered there in 1802 and 1805, and for the 1802 journey there have survived at the Historical Society of Pennsylvania several manuscript books that give the details of his journey, accounts of Warm Springs, Natural Bridge, etc.[42] Also in his herbarium there were the 900 specimens gathered by Frederick Pursh, nearly all of them from Virginia. Why not prepare a new Virginian Flora?[43] We have already seen how highly Barton esteemed Clayton's descriptions that had appeared in the old *Flora Virginica*, given out by Gronovius in Holland. Believing this now unavailable work of such unique value that it should be in every student's hands, Barton reprinted its Latin text of 1762, even presenting its title-page after his own; then to each species he added new matter in English, his insertions being distinguishable by asterisks. Thus, you have at one view the old and the new. Only 74 pages, merely the beginning, of Barton's *Flora Virginica* were ever published. This was in Philadelphia in 1812. Apparently copies were not definitely distributed, although such exist in at least three libraries of this city, the American Philosophical Society, the Academy of Natural Sciences, and the Library Company of Philadelphia. I do not know of the book elsewhere.

Though so often speaking of his plans, Barton was secretive about their fulfillment, so that rumors could develop as to his doings. In April, 1813, the veteran Pennsylvania botanist Henry Muhlenberg wrote to the young Dr. Baldwin, saying "Dr. Barton, it seems, has published the first 8 classes of his Flora,—but has sent all the copies to England; and none are left for the American Botanists who want a Flora most."[44] Two years later that is what Barton did; he went to Europe, and carried with him most of the copies of his *Flora Virginica*. One wonders why. In my earlier study I surmised that he hoped to obtain assistance on the grasses and sedges, which, following the Linnaean classification, would have

[42] "Journal of Benjamin Smith Barton on a visit to Virginia, 1802," edited by W. L. McAtee, in *Castanea*, 3: 85–117, 1938.

[43] Letters of 1792 and 1793 from James Greenway of Dinwiddie County, Virginia, preserved at "Montgomery Place," show that he was projecting a "Flora Virginica," while another from James Cooke in 1809, after Dr. Greenway's death, states that his manuscripts are being deemed by his heirs as "a treasure of inestimable value." Whether Greenway's "Flora" was acquired by Barton, and contributed anything to his own needs investigating. Incidentally, Greenway also expressed himself vigorously on the Clayton versus Gronovius situation of the classic *Flora Virginica:* "Clayton . . . underwent the Labour of the Pack-Horse and Gronovius reaped the Profit and Credit of the Whole."

[44] Darlington's *Reliquiae Baldwinianae*, p. 78, 1843.

been next before him, but that would not have required so much of his stock unless he planned to have this part printed and inserted abroad. His former student, Pursh, who had just published his *Flora Americae Septentrionalis,* seemed the likely botanist to help him, since Pursh must have covered plants of Virginia in his great work; perhaps he was appealed to, but against this is the fact that, although in his *Flora* Pursh had amply acknowledged his indebtedness to Barton, the Professor had not been so kind to his former assistant. Barton omitted Pursh's name in the introduction to his *Flora Virginica,* and alludes to his aid only in the remark: "I have derived great assistance from the large herbaria which have . . . been collected at my expense." Indebted as Barton's *Flora Virginica* must stand to Pursh's field and herbarium work, the obligation is none too graciously acknowledged. Finally, did copies of the *Flora Virginica* come back to America with Barton?

It was in April, 1815, that Professor Barton set sail for France, the sea-voyage being for his health, his nephew tells us. He saw Cuvier and others in Paris, and then had a period of visits in England. But in England he was repeatedly sick, and on the return voyage landed in New York so ill that "for three weeks he was despaired of." This was in early November, and it was there and, by the hands of various amanuenses, that he prepared memoranda[45] as to the disposition of his scientific effects and papers. The memorandum of November 8 leaves to Cuvier at Paris all his fossils, his drawings and descriptions of mammals, and drawings of the heads of Indians. That of November 9 urgently requests that his "two extensive manuscripts on the life and writings of Dr. Priestly[46] and on the Insects of the United States . . . be *not* presented to the American Philosophical Society or on any account to be given to the Public. Let these manuscripts be preserved . . . by my family and friends." But the Philosophical Society was to have copies of all his printed works. The memorandum of November 11 informs "any friend or a bookseller, disposed to make a collection of my scattered essays," where to find them. That of November 12 is concerned with the sale of various of his papers. Finally, that of November 21, which is from Princeton, gives notes for his editor concerning the reprinting of certain of his papers.

Only three days before his death, so his nephew tells us, Barton wrote a scientific paper, certainly his last. It was entitled "Miscellaneous Facts concerning the first Discovery of the genus of *Bartonia,* accompanied with a drawing of one of the Species." The younger Barton, at his uncle's request, then made the drawing and also read the memoir at the next ensuing meeting of the Philosophical Society.[47] It deals with a splendid plant that had been illustrated in color in the *Botanical Magazine,* where it had been published by Pursh as a new genus *Bartonia* in honor of his early patron. Nuttall, then in England, had promptly written Barton, stating that he had himself proposed the name, a fact that Barton could verify from specimens, seeds, and manuscript book of descriptions that Nuttall had forwarded to him from New Orleans, before going to England. So Barton wished to make clear the fact that it was Nuttall and not Pursh who had really proposed this name, and that Nuttall's name *Bartonia superba* should be used rather than Pursh's *B. ornata* (or, as he first had it, *decapetala*). But why was Barton so concerned about this rendering of credit to Nuttall? I cannot but suspect that Nuttall had been to visit his old patron, a contact that we have seen was impossible earlier, and that Barton, in his sickness, had relished the personal attention. But although he spoke with satisfaction of this "opportunity of doing some justice to the zeal and services of Mr. Nuttall . . . to whom the work of Frederick Pursh is under infinite obligations," he quite justly added: "I might say something more in this place, concerning my claims to the collections of these two travellers and botanists; but this is rather a matter which respects the individuals concerned than the Public."

The worn professor, conscious that he was at last through with his projects, so many of which he was leaving unfinished, must have felt some comfort in the thought that his name was to be

[45] Now at the Historical Society of Pennsylvania.

[46] Dr. Barton, as elected by the American Philosophical Society, delivered on January 3, 1805, his "Eulogium on Priestley," the famous chemist and English liberal who had lived his last years in Pennsylvania. A copy of the address was immediately requested for publication by the Society.

[47] W. P. C. Barton expected this paper to appear in the next ensuing volume of the Society's *Transactions,* but it has never been published and remains as a manuscript in your Library. He himself drew from it so as to explain, in his uncle's obituary sketch, how the latter had aided Nuttall. In *Bartonia,* **18**: 49–51, 1936, I have also drawn from it to make clear the competition about *Bartonia.*

perpetuated in a flower so striking. But he reckoned without priority, the fetish of modern naturalists. Years earlier Muhlenberg had suggested *Bartonia* for a small-flowered and singularly insignificant member of the gentian family, and it had been duly published in 1801 by the German botanist, Willdenow. Slight as are its claims to distinction, Muhlenberg's *Bartonia* at least has the merit of growing in the territory that Barton knew personally. Turpin's drawing of it for the *Prodromus,* as found at "Montgomery Place," is in our exhibition to-day.

All in all it was for neither of his floras, which remained unfinished fragments, that Barton deserves to be remembered in systematic botany, but for his patronage of Frederick Pursh and Thomas Nuttall, two of the ablest taxonomists who have studied the flora of this continent. If we are warranted in adding Thomas Horsfield to the list of those who owed an early impetus to him, we have Barton's influence carried far overseas to the luxuriant flora of the East Indies.

Benjamin Smith Barton died December 19, 1815, in his fiftieth year. The only contemporary obituary I know is that which was prepared by his nephew Dr. William P. C. Barton, his brother William's elder son, and which was read by him before the Philadelphia Medical Society February 16, 1816. Dr. Caspar Wistar, it seems, intended to write a fuller account of the life and work of the elder Barton, evidently feeling that his achievements merited it. To us of a later time, Barton was greater in intentions than in practice, in the fields that he perceived should be cultivated than in his ability to accomplish the needed tasks. But, if he is the "Father of American Materia Medica," he should be hailed as the "Father of American Botanical and Zoological Instruction," and I suspect as the "Father of the Scientific Study of the American Indian" as well.

In his *Biographical Sketch* of his uncle the younger Barton does not tell us something that he later felt constrained to put upon printed record. He had told his audience, however, a little more than I have yet told you, for he stated that his uncle had married in 1797, and was survived by a widow and two children—his wife was Mary Pennington, and the children were named Thomas Pennant and Hetty. The thing which the younger Barton told only later, was that, even in the preparation of his uncle's obituary, he had been denied all access to his uncle's papers; indeed, he feared that these might have been "recklessly destroyed."[48] Of course there were some manuscripts at the American Philosophical Society and elsewhere, but most of what was unpublished and all the wealth of memoranda that the younger Barton knew had existed, had apparently gone. Nor did any of it reappear during his lifetime. Thomas Pennant Barton, Benjamin's son, grew to manhood, espoused the daughter of Edward Livingston of New York, and accompanied his diplomatic father-in-law to Paris; on returning to this country he and his wife settled at "Montgomery Place," the Livingston estate on the Hudson River, where he assembled an outstanding library. After his death in 1869 his widow, Cora Livingston Barton, sold his vast collection of books, with its rare Shakespeareana, to the Public Library of Boston, and I doubt not that it was she who gave to the Historical Society of Pennsylvania its collection of Bartoniana, the manuscript books and letters to which I have so often referred. The bound volume of letters there commences with a pamphlet eulogy of Mrs. Edward Livingston, who was Cora Barton's mother.

Were I writing this account ten years ago, I should have said that the tale of the elder Barton's

[48] "It is highly probable that among the manuscripts which were left by the late Professor Barton, much interesting information on this subject might be collected. His well known inquisitiveness, and his constant habit of recording, in however desultory a manner, the facts with which his enquiries made him acquainted, warrant the belief, that the public are deprived of some curious and very interesting knowledge, in consequence of the cloak which has been thrown over his collections. Strange as it may appear, it is not the less true, that not a single one of these manuscripts, not even his lectures on Materia Medica, has ever seen the light. The public is yet to be informed what has become of the industrious collections of that eminent man; and it is sincerely hoped that they have not been recklessly destroyed. Though the author of this work, his own nephew, was engaged in the same pursuits which occupied much of the time and attention of the late Professor, not a single line of his manuscripts has ever been put into his hands, or seen by him; nor was his opinion even asked about the disposition of them. But on the contrary, he was refused a sight of such memorandums and notes as were asked for to enable him to write an authentic account of his life, when called on to do so by the Philadelphia Medical Society, of which the Professor, at the time of his decease, was president. It has been deemed proper to make this public avowal, because it has been mentioned to the author, that some persons supposed him possessed of all the papers and collections of the late Professor Barton, designing by such intimation, to deprive the author of whatever credit his persevering exertions, in despite of discouraging and opposing obstacles, may have deserved." Footnote in *Medical Botany* ("Vegetable Materia Medica of the United States"), **2**: viii, 1818.

lost papers ended here. But it does not. Mrs. Barton died, and the Livingston estate reverted to her cousins of the Livingston name. Eventually in 1921 it passed to descendants who expected to be with us to-day, General and Mrs. John R. Delafield. "From the time that ["Montgomery Place"] came into our possession," writes Mrs. Delafield in *Bartonia*,[49] "we realized that large numbers of papers and documents belonging to former residents, were stored everywhere, in odd nooks and corners, in the study, and in cases in the attic. One of the chests contained papers, proof sheets, letters, etc. which had belonged to Dr. Benjamin Smith Barton. . . . Besides the chest . . . we found an old portfolio in another box hidden under the overhanging eaves of the attic. . . . It contained many of the original drawings made by William Bartram for *The Elements of Botany*. . . . There were besides many colored drawings by Turpin, W. P. C. Barton, Dr. Barton himself, and other artists which were probably intended to illustrate other works. There were also copper plates made for the engravings.'" The items could be numbered by the hundred, and they were all direct from Benjamin Smith Barton.

From what I have seen of this discovery, only a tiny portion of which has been placed on exhibition[50] in the room above, it seems certain to shed new light, not only on Barton himself but on the intellectual life of Philadelphia during a period when it held unquestioned leadership in the nation.

[49] *Bartonia*, **18**: 51, 1936.

[50] In the Members' Room of the Society there were displayed various items pertaining to Benjamin Smith Barton. From the Society's own collections deposited at the Academy of Natural Sciences of Philadelphia these included a bound volume of plant specimens, dated 1795, gathered by Barton himself, André Michaux, Major Adlum, and others; separately mounted collections by B. S. Barton in crossing New York State in 1797, in Virginia in 1802, etc.; and such collections made for Barton by various pupils, and especially by his assistants, Frederick Pursh and Thomas Nuttall. From "Montgomery Place" and kindly lent by General and Mrs. John R. Delafield, there were shown manuscripts, proofs, and drawings in B. S. Barton's hand, being a field diary of his 1797 trip across New York, a meteorological diary, a list of medical students, papers endorsed "Relating to my Life and Pursuits," the dictated draft of his last paper (on *Bartonia*), proof pages of his lost *Prodromus* and of his unfinished "Geographic distribution of trees and shrubs," and drawings of fossil teeth and of anatomical structures; while, made by others for him, were drawings by P. J. F. Turpin of two plants, *Jeffersonia* and *Bartonia*, by William Bartram of both plants and animals, and by W. P. C. Barton (as an engraved print) of a horned toad. The nine drawings made by William Bartram, some of which were for Barton's *Elements of Botany* but others for works never published, are especially beautiful.

ENTOMOLOGY, SCIENTIFIC AND HUMAN ASPECTS

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(*Read February 13, 1942, in Symposium on the Early History of Science and Learning in America*)

In the "Complete roster" of our Society, published in 1930, the first name is, of course, that of Benjamin Franklin. Paul Leicester Ford's *The Many-Sided Franklin* (1899), neither in its index nor in the chapter on Franklin as a Scientist, gives any hint that Franklin concerned himself with entomology. An examination of the Index to Albert Henry Smyth's edition of the *Writings of Franklin*, however, reveals Franklin's interest in 1769–1772 in the attempts of our fellow member, Cadwalader Evans, to establish silk culture in Pennsylvania. More to our present point is Franklin's own account of his experiments on resuscitation of drowned flies. Some time subsequent to April 15, 1773, Franklin wrote to Barbeu Dubourg, a physician and Franklin's French translator, on the general subject of causes of death and experiments for recalling to life those who appear to be killed by lightning. He continues: "I have seen an instance of common flies preserved in a manner somewhat similar. They had been drowned in Madeira wine, apparently about the time when it was bottled in Virginia, to be sent hither (to London). At the opening of one of the bottles, at the house of a friend where I then was, three drowned flies fell into the first glass that was filled. Having heard it remarked that drowned flies were capable of being revived by the rays of the sun, I proposed making the experiment upon these; they were therefore exposed to the sun upon a sieve, which had been employed to strain them out of the wine. In less than three hours, two of them began by degrees to recover life. They commenced by some convulsive motions of the thighs, and at length raised themselves upon their legs, wiped their eyes with their fore feet, beat and brushed their wings with their hind feet and soon after began to fly, finding themselves in Old England, without knowing how they came thither. The third continued lifeless till sunset, when, losing all hopes of him, he was thrown away.

"I wish it were possible, from this instance, to invent a method of embalming drowned persons in such a manner that they may be recalled to life at any period, however distant; for having a very ardent desire to see and observe the state of America a hundred years hence, I should prefer to any ordinary death, the being immersed in a cask of Madeira wine, with a few friends, till that time, to be then recalled to life by the solar warmth of my dear country! But since in all probability we live in an age too early and too near the infancy of science to hope to see such an art brought in our time to its perfection, I must for the present content myself with the treat, which you are so kind as to promise me, of the resurrection of a fowl or a turkey cock."[1]

Dubourg included this letter in his *Oeuvres de M. Franklin* (1773).[2] One wonders why. Was it as an example of Franklin's humor? Or did he seriously accept the suggestion that the flies survived a transatlantic voyage in wine, of perhaps two months plus, and did not wish to cast any imputations on the cleanliness of the English glasses into which wine and flies were poured? This fly story is, alas, not satisfactory to the entomologist of to-day. Dubourg's only comment is a footnote which we translate as follows: "A number of tipplers would willingly adopt this new kind of burial; but only after their death."

But if we cannot add an entomological halo to the many coronas already encircling the head of our illustrious founder, the case is otherwise with the person whose name is second on the same roster. It is that of John Bartram. We think of Bartram as Botanist to the King, but between 1746 and 1763 he sent five brief articles on insects to his correspondent, Peter Collinson, in London, through whom they appeared in the *Philosophical Transactions of the Royal Society*. They deal with wasp nests made of clay, the habits of the

[1] This translation of the letter to Dubourg is in Jared Spark's edition of the works of Franklin, Boston: Hilliard, Gray & Co., **6**: 381–383 (1838) (if not earlier), and in later editions by other editors. Also in Albert Henry Smyth's *The Writings of Benjamin Franklin*, The Macmillan Co., **6**: 43–44 (1906).

[2] Vol. I, pp. 327–328.

great black and the yellowish wasps, the dragonfly and the may-flies, all of Pennsylvania. They are mixtures of Bartram's observations and Collinson's additions, so that it is not always easy to identify the authorship of each sentence. Illustrations were added, taken in some cases from their great French contemporary, Réaumur. Three of them were translated into German and published in that dress in Germany.[3] There is also a posthumous paper by John Bartram on the Seventeen-Year Cicada, published in Barton's *Philadelphia Medical and Physical Journal* in 1805. His son, Moses Bartram, had previously published observations on the same insect in the *Annual Register of London* for 1767.

The literature on and of American entomology from 1588 to 1865 has been summarized by Harry B. Weiss, Chief of the Bureau of Plant Industry of the New Jersey State Department of Agriculture, in a mimeographed volume of 320 large pages, entitled "The Pioneer Century of American Entomology," published by the author, New Brunswick, New Jersey, 1936. It deserves, and still awaits, republication in a more permanent form. To it I am much indebted in the preparation of the present article.

Weiss (p. 28) has found a passage in Bartram's writings reading thus: "As for the animals and insects, it is very few that I touch of choice, and most with uneasiness. Neither can I behold any of them, that have not done me a manifest injury, in their agonizing mortal pains without pity." Bartram and other writers of his time included reptiles among insects. Even the great Réaumur, in 1734,[4] wrote that he willingly accords the term insect to all animals other than quadrupeds, birds and fishes. "A crocodile would be a furious insect, but I would have no trouble in giving it this name."

John Bartram was not the first native-born American to write on American insects; he was preceded by Paul Dudley of Roxbury, Massachusetts (1723), on bees in the woods and William Byrd of Virginia on insects vexatious to man; possibly also by Nathaniel Moreton of New England (1669) on cicadas, Benjamin Bullivant of Boston (1698), Thomas Matthews of Virginia (1705), also on cicadas, and Richard Lewis of Annapolis, Maryland (1735), if these last four were native-born. Byrd's and Matthews' notes were not published until long after Bartram's death, however.[5]

John Bartram's papers above mentioned, his books of travels in Pennsylvania (1751) and in East Florida (1769), his son William's *Travels* in the Southern States (1791), Moses Bartram's paper just quoted and one in the first volume of our own *Transactions* on our native silk worms, also by Moses Bartram, in so far as they deal with insects, are of the pure and undefiled brand of entomology.

But as was to be expected in a new country and in a Society whose complete name emphasized *Useful* Knowledge, the majority of the papers on insects read before our Society up to 1817 were of the sort now referred to as Economic Entomology and of most of them we have titles only. The first of these was by Landon Carter, of Sabine Hall, Virginia, entitled "Observations Concerning the Fly-Weevil that destroys the Wheat, with some useful discoveries and conclusions, concerning the propagation and progress of that pernicious insect and the methods to be used to prevent the destruction of the grain by it." This was ordered to be published November 15, 1768, and it occupies twelve pages in the first volume of our *Transactions*.

Carter's title is puzzling to the modern reader. In his text he speaks of "this little destructive insect called the Moth or Fly-Weevil. . . . It is with much propriety called a Weevil as it destroys the wheat even in our granaries, though it is not of the kind termed by naturalists the Curculio." A little farther on he says "They are a pale brownish moth." Carter's insect was identified long ago as the Angoumois Grain Moth (*Sitotroga cerealella*), called, by one of our most recent text-books on destructive insects, "the most destructive grain moth occurring in this country." Carter says that he had heard of this insect more than twenty-five years previous to his paper, that is not later than 1743. As to his method to prevent destruction of the grain by the fly-weevil, Carter tells us: "I fell solely upon the exclusion of the air as much as possible; and this I was happy enough in effecting with great success to my crop for many years, till my old age and infirmity prevented my attending my servants whilst they were pursuing my directions; and not till then had I the least reason

[3] Hagen, *Bibliotheca Entomologica*, 1862–63, under Bartram. Horn u. Schenkling, *Index Litteraturae Entomologicae*, Serie I, 1: 46, 1928.

[4] *Memoires pour servir a l'histoire des insectes*, I, pp. 57–58.

[5] Marlatt, *The periodical cicada*. Bull. 71, Bur. Entom., U. S. Dept. Agric., 147, 154 (1907). Weiss, *The Pioneer Century*, 14, 25–27.

to complain, whilst others were eaten up in their crops; but villainy and negligence are such concomitants in servitude, that I have been again destroyed, plainly to be accounted for from the visible abuse of my constant directions. The method I have taken is this. I reap as early as I possibly can, respecting the dryness of the grain, as well as the straw, which would otherwise funk it: At least two feet quite round the mow I leave a vacancy, which is to be well trod with soft hay, or beaten straw; therefore I keep persons constantly treading down those margins as the mow rises; and when I reach the eves of my barn, I lay on and tread down a very thick covering of the same straw or hay, and weighed it well down at the top." He was "convinced that the exclusion of air, as much as possible, could be the only effectual method of killing those eggs, which as they are so very small before hatching, could not give the least disagreeableness in either look, taste or quality, in the flour."[6]

Dr. G. Brown Goode, Assistant Secretary of the Smithsonian Institution, in his presidential address to the Biological Society of Washington in 1888, says: "Organized effort in economic entomology appears to date from the year 1792, when the American Philosophical Society appointed a Committee to collect materials for a natural history of the Hessian fly, at that time making fearful ravages in the wheat-fields and so much dreaded in Great Britain that the import of wheat from the United States was forbidden by law. The Philosophical Society's Committee was composed of Thomas Jefferson, at that time Secretary of State in President Washington's cabinet, Benjamin Smith Barton, James Hutchinson and Caspar Wistar. In their report, which was accompanied by large drawings, the history of the little marauder was given in considerable detail."[7]

On consulting the volume of printed summaries of the minutes of the Society from 1744 to 1838 it appears that this Committee was appointed at the meeting of April 15, 1791 (pp. 193–194), on Jefferson's motion and included also a Mr. Thompson, not further identified. Jefferson was in that year one of the three vice presidents of the Society. The same printed volume seems not to mention the report of this Committee.

At the beginning of the nineteenth century the Society offered a prize of $60 for the best method of preventing the premature decay of peach trees. Two of the papers submitted were of such equal merit that the prize was divided equally between the two authors. John Ellis of New Jersey was one of the prize-winners and his paper entitled "Account of a Method of preventing the premature decay of peach trees" was printed in our *Transactions* for 1802 (Vol. 5, appx. No. 1, 325–326).

It dealt with the peach-tree borer. His method was "to clear away the dirt as to expose the root of the tree to the depth of three inches" and to then apply a band of straw one inch thick and three feet long, binding around the tree at three places, and having the band extend to the bottom of the hole. The soil was then replaced. Upon the approach of frost the straw was removed. By this method egg-deposition was prevented within three feet of the root. Ellis tried this method on numerous trees and found that it worked. The other prize-winner was Thomas Coulter of Bedford County, Pennsylvania."[8] His paper says nothing of insects and is concerned with methods of cultivation.

Treating of the period which we have now reached, Weiss says: "In 1795 there appeared an entomological contribution from our first American entomologist worthy of the name. This was printed in the *Massachusetts Magazine* (Vol. 7, Sept., pp. 323–327, Oct., pp. 415–416, 1 pl.) under the title 'The description and history of the canker worm' and the author was William Dandridge Peck." Peck has six entomological papers to his credit, all of them on insects injurious to vegetation, as against two on honey and the honey bee by Benjamin Smith Barton, his Pennsylvanian contemporary. Barton published also *Fragments of the Natural History of Pennsylvania* in 1799, reprinted by the Willughby Society in England in 1883; it is concerned chiefly with birds, but gives some notes on insects observed at Philadelphia in 1791. Both Barton and Peck were elected members of our Society, the former in 1789 (Roster, no. 496) the latter in 1796 (no. 606), and Barton received the Society's Magellanic Premium in December, 1804, for a paper on a "Number of the Pernicious Insects of the United States"[9] which seems never to have been published. Barton was a curator of the Society from 1790 to 1800 and one of the three vice presidents from 1802 to 1815.

Entomologists in those days were not always able to examine their insects minutely. Dr. Hugh

[6] *Trans. Am. Philos. Soc.*, **1**: 284 (rev. ed.) (1789).
[7] Goode, *Proc. Biol. Soc. Wash.*, **4**: 39 (1888).
[8] Weiss, *Pioneer Century Amer. Ent.*, p. 58. *Proc. Amer. Philos. Soc.*, **22**, pt. 3: 303–304 (1884), meeting of October 3, 1800.
[9] *Proc.*, **22**, pt. 3: 369 (1884), meeting of Dec. 21, 1804.

Williamson (Roster No. 51) of North Carolina read a paper to our Society, Feb. 1, 1799, "On the Ephoron leukon, usually called the White Fly of Passaick River," in which occurs the remark, "Neither mouth nor feet could be described from the want of a microscope." This insect is one of the mayflies and Williamson's name for it is employed in the latest manual of this group.[10]

A very definite step was taken in the cataloguing of American insects when, in 1806, Friedrich Valentin Melsheimer published at Hanover, York County, his "Catalogue of Insects of Pennsylvania, Part First," listing 1363 species of Coleoptera. Melsheimer came to America as a chaplain in the Brunswick dragoons, forming part of Burgoyne's army, and was taken prisoner by the American troops at the Battle of Bennington.[11] Eventually he settled at Hanover where he describes himself as "Minister of the Gospel." In 1795 he was elected a member of our Society. His interest in entomology and especially beetles was continued by his sons, John Frederick and Frederick Ernst, who successively inherited their father's collection and library. The latter sold them to Louis Agassiz in 1864 and they form an important part of the entomological collection of the Museum of Comparative Zoölogy of Harvard College. In 1937 a bronze memorial plaque to F. V. Melsheimer, including the statement "Known to Science as Father of American Entomology," was unveiled on the lawn before the High School at New Holland, Pennsylvania.[12]

One of the Melsheimers' correspondents, both of father and of son, was Thomas Say of Philadelphia. Say became the best-known American entomologist of his day, although his publishing activity in that field occupied only seventeen years—from 1817 to 1834, the year of his death at New Harmony, Indiana, at the early age of forty-seven. Say's writings on insects to the number of thirty-one, including three which appeared after his death, were collected in two volumes, totalling 1226 pages, by John Lawrence Le Conte (of whom more later) and published in 1859. In 1873, Le Conte wrote:

> The beginning of the American school of entomology may be considered as made in 1817 by Thomas Say, in those days the most generally instructed zoologist in the United States . . . the entomologists of that early period were essentially *species men*, namers and describers of the unknown objects with which they were surrounded:—a work which was done so well that of the many hundreds of species described by Say & the smaller number by his collaborators, scarcely any remain doubtful and but few unknown.[13]

In 1931, Dr. L. O. Howard wrote: "In fact, Say ranks to-day as the most eminent of the early American taxonomic zoologists." In these recent years Say has found sympathetic biographers in Harry B. Weiss and Grace M. Ziegler to whose joint work Dr. Howard contributed a foreword from which we have just quoted. These biographers tell us that Say described 1150 species of Coleoptera, 225 of Diptera, 100 of Hemiptera and 100 of other insects, a total of 1575. He published also on fresh-water, marine and land shells, both recent and fossil, on mammals, birds, reptiles and Crustacea.

Say was elected a member of our Society in 1817 and was curator from 1821 to 1827. A memoir of Say by his associate, George Ord, was read to our Society December 19, 1834, a little more than two months after his death, but remained unpublished until the appearance of Le Conte's collected edition of Say's works.

Previous to his election into our Society, Say had taken part in the foundation of the Academy of Natural Sciences of Philadelphia in 1812, to which he naturally gave more time and attention than to the Philosophical Society. Of his papers on insects four were published in the *Transactions* of the Philosophical Society, 1818–1839, and thirteen in the *Journal* of the Academy, 1817–1839.

Weiss and Ziegler repeat the story that after a financial loss, about 1812, Say "gave himself entirely to natural history, living in the rooms of the Academy, on the plainest food, usually bread and milk, with an occasional chop or an egg which he himself cooked, and sleeping, so it has been said, under the skeleton of a horse. He continued in this style of living for several years and at times his food did not cost him more than an average of 12c a day. He was so immersed in

[10] Needham, Traver, Yin-Chi Hsu, *The biology of mayflies*, 244 (1935).

[11] Melsheimer's autobiography in Herbert Osborn's *Fragments of Entomological History*, 22–23 (1937); Osborn has brought together much information on Melsheimer. There is also an article, not without some errors, on F. V. Melsheimer by M. Luther Heisey, *Papers* of the Lancaster County Historical Society, **41** (4): 103–112, portrait, reproduction of tombstone, signature, bibliography and most of the autobiography (1937).

[12] *Lancaster Intelligencer-Journal* for Aug. 30, 1937, reproduced in *Entom. News*, **48** (8): 231–232 (Oct., 1937).

[13] *Amer. Naturalist*, **7**: 711–712.

his scientific work that he was said to have expressed facetiously the wish that he had a hole in his side in which his food requirements could be deposited so that his work need not be interrupted by eating."[14] They also tell us that "it is known that he attended the Wistar parties, the social events of the members of the American Philosophical Society,"[15] although without specifying the year, and they quote a contemporary account of these parties by Capt. Thomas Hamilton as follows: "The conversation is generally literary or scientific, and as the party is usually very large, it can be varied at pleasure. Philosophers eat like other men, and the precaution of an excellent supper is by no means found to be superfluous. It acts too as a gentle emollient on the acrimony of debate. No man can say a harsh thing with his mouth full of turkey, and the disputants forget their differences in unity of enjoyment."

The mouth-full-of-turkey days for Thomas Say presumably came later than those of twelve cents a day at the Academy.

Say has been widely termed the Father of American Entomology but Say himself ascribed this honor to F. V. Melsheimer.[16] Prof. Herbert Osborn, of Ohio State University, has proposed to solve this problem in genetics in this way: "As it has been a common practice to call Thomas Say the 'Father of American Entomology' it may be appropriate to call F. V. Melsheimer the 'Grandfather of American Entomology'."[17]

Among Say's correspondents on entomological subjects was Dr. Thaddeus William Harris, born in Dorchester, Massachusetts, in 1795, student at Harvard (where he heard Peck's lectures on natural history), temporary professor of natural history and librarian for twenty-five years at his alma mater to his death in 1856. His catalogue of insects of Massachusetts in 1833 embraced 2350 species and is described by Weiss as the first attempt in this country to enumerate and classify American insects on a large scale. He is chiefly remembered for his Report (1841), later Treatise (1842), on insects injurious to vegetation, prepared and published at the expense of the State of Massachusetts. Dr. L. O. Howard[18] says of Harris: "American entomologists of to-day may well think with pride of the man who was really the founder of applied entomology in this country." Harris appears to have had no connection with our Society but his importance in the history of American Entomology justifies his being mentioned here.

Two years after Thomas Say's election to our Society, or in 1819, Pierre André Latreille was elected a member; one wonders whether he was suggested by Say. He was styled the Prince of Entomologists and was the author of the section on the Articulata in Cuvier's *Regne Animal.*

From 1819 until 1833 we recognize no entomological name in the roster of our members, but in the latter year appears that of Titian R(amsey) Peale, a son of Charles Willson Peale, artist and founder of the Philadelphia Museum. Titian Peale published a *Lepidoptera Americana* in 1833, which never got beyond its first number of fourteen pages and four colored plates; he illustrated various works on natural history and was a member of the Wilkes Antarctic Expedition.[19]

Louis Agassiz was elected to our Society in 1843, one year before Samuel Stehman Haldeman and six years before Joseph Leidy. At the time of his election, Agassiz had not arrived in America nor had he published upon insects, but five papers thereon came from his pen in later years. He was known at the time of his election for his work on fossil fishes and on glacier geology.

Haldeman became author of thirty-two papers on insects between 1842 and 1859, chiefly descriptive of Coleoptera. In 1842 he, Frederick Ernst Melsheimer, Daniel Ziegler and John G. Morris, all Pennsylvania-born, formed at York the Entomological Society of Pennsylvania, apparently

[14] *Thomas Say,* 35 (1931). The original source of these anecdotes of Say has eluded me. They are not in Benjamin H. Coates' biographical sketch read to the Academy Dec. 16, 1834, and published separately in 1835, nor in Ord's memoir above mentioned, nor in J. G. Morris's "Contribution toward a History of Entomology in the United States" (*Amer. Jour. Sci. and Arts,* 2-1: 17–27, 1846). The earliest place in which I have found them is in William Parker Foulke's "Discourse in Commemoration of the Founding of The Academy of Natural Sciences of Philadelphia," delivered March 20, 1854, in the hall of the University of Pennsylvania, Philadelphia. Printed for the Academy 1854. In repeating them, Foulke says they "must be familiar to many of you," but gives no source. Perhaps some reader of this footnote can supply the lacking information.

[15] *Thomas Say,* 113, 115.

[16] Say's exact expression is: "The late Rev. F. V. Melsheimer the parent of entomology in this country and a very industrious collector." (*American Entomology,* 1817, second unnumbered page for Plate II, *Geotrupes tityus*). Morris, in his article of 1846, quoted in footnote 14 above, says (p. 18): "The father of American entomology is the Rev. F. V. Melsheimer."

[17] *Fragments of Entomological History,* p. 19, footnote.

[18] Howard, "A history of applied entomology." *Smithson. Miscell. Colls.,* **84**: 35 (1930).

[19] *Ent. News,* **24** (1): 1–3 (1913).

the first entomological society in America, but which endured for only a few years.

The claiming of Dr. Joseph Leidy as an entomologist rests chiefly upon two anatomical papers dealing respectively with the giant water bug *Belostoma* (1847) now known as *Lethocerus,* and *Corydalus* (1848), the larva of which is called dobson and hellgramite. Leidy was author also of shorter notes upon various insects.

The chief significance of Louis Agassiz in the history of biology in the United States lies in the enthusiasm which he aroused in his students. Some of these students became distinguished as entomologists and among them we may cite John Lawrence Le Conte, Philip Reese Uhler, Samuel Hubbard Scudder, Alpheus Hyatt, Charles H. Fernald, Alpheus Spring Packard, Andrew John Cook, Edward Burgess and Charles Sedgwick Minot. Le Conte, Packard, Scudder, Hyatt and Minot were in time elected into the membership of our Society, although not all of them on the basis of entomological work.

John Lawrence Le Conte was the oldest of this list, born in New York City in 1825, in the same year as Huxley and two years after Leidy. He was eighteen years younger than Agassiz and although perhaps not as truly Agassiz's student as others on this list, he recognized his debt to the great Swiss zoölogist.[20] He was the son of an entomologist, Major John Eatton Le Conte of the Engineers Corps, United States Army, author of papers on Coleoptera and coadjutor of Jean Baptiste Alphonse Boisduval on the *Histoire generale et iconographique des Lepidoptéres et des chenilles de l'Amerique septentrionale,* published at Paris in 1833. Major Le Conte was the John Le Conte elected to our Society in 1851 (No. 1287).

John Lawrence Le Conte has been considered by many to be the greatest entomologist whom America has yet produced. We have already quoted his characterization of Say as a species man, but Le Conte far outdid Say in this respect. He is stated to have described or named 4820 species and 514 genera of beetles.[21] His true pre-eminence, however, lies in his recognition and definition of the larger groups of Coleoptera. His interests were not only in taxonomy but also in zoögeography. The Le Contes moved from New York to Philadelphia in 1852. Dr. John Lawrence Le Conte (he had the M.D. of the College of Physicians and Surgeons of New York) served our Society as a Secretary from 1855 to 1879 and as a Vice President from 1880 until his death in 1883. He was President of the American Association for the Advancement of Science in 1874; he and Dr. L. O. Howard have been the only entomologists to occupy this position.

Alpheus Spring Packard was also the holder of an M.D. (from Bowdoin College). He was professor of zoology and geology at Brown University from 1878 until his death in 1905. Of all of Agassiz's entomological students he departed farthest from the theoretical views of his teacher and, with Anton Dohrn, Friedrich Brauer, Fritz Müller and Sir John Lubbock, took a prominent part in carrying out the Darwinian ideas in the search for the ancestry of insects. He was the author of three text-books, the *Guide to the Study of Insects* (1869 and later editions), *Entomology for Beginners* (1888), and *A Text-Book of Entomology* (1898), all influential. He was one of the founders of the *American Naturalist.* His most extensive entomological work was on the Lepidoptera. His papers on all subjects number 579.[22]

Samuel Hubbard Scudder, who came to Agassiz to study butterflies and was given a fish, as the late Mrs. Lucy Langdon Williams Wilson impressed on her pupils in the Philadelphia Normal School, was born in Boston in 1837 and died in Cambridge in 1911. He compiled a universal index to the genera in zoology, wrote catalogues, guides, revisions and descriptions of Orthoptera, published a magnificent account of the *Butterflies of the Eastern United States and Canada* and made known many fossil insects, arachnids and myriopods of Europe and of North America. He is credited with the authorship of 791 scientific papers.[23]

Alpheus Hyatt's work lay mostly outside the insects, as in the fossil cephalopods, but with J. M. Arms he wrote a suggestive introduction, "Insecta," 1890. Charles Sedgwick Minot is chiefly known as a vertebrate embryologist, but in his earlier years he described butterflies and moths, wrote notes on their habits, studied the histological structure of tracheae and of some Orthoptera and Lepidoptera.

Philip R. Uhler, Librarian (1870–1880) and Provost (1880–1911) of the Peabody Library of

[20] *Amer. Nat.,* **7**: 713.

[21] Scudder, *Trans. Amer. Ent. Soc.,* **11**: xvii, but see Henshaw, same *Trans.,* **9**: 270 and 272.

[22] Cockerell, "Biographical Memoir of Packard," *Nat. Acad. Sci. Biog. Mem.,* **9**: 236, 1920.

[23] Mayor, *Mem. Nat. Acad. Sci.,* **17**: 104 (1924).

Baltimore, became the leading authority in the United States on the Hemiptera. A. J. Cook was active in economic entomology in Michigan and in California. Burgess furnished an excellent and oft-quoted account of the anatomy of the milkweed butterfly and also designed yachts as the Puritan, the Mayflower, the Volunteer, to defend the America Cup in 1885, '86 and '87.

John Lawrence Le Conte's work on Coleoptera was supplemented and continued by his friend and associate, George Henry Horn, a native Philadelphian, elected to our Society in 1869, one of our Curators from 1881 to 1886 and a Secretary from 1887 until his death in 1897. He described 1582 species and 154 genera of beetles, some of which, as in the case of almost all taxonomists, have been sunk in the sea of synonymy.[24] It would appear that Thomas Say, John Lawrence Le Conte and George H. Horn together are responsible for names and descriptions of 7552 species of beetles.

Another contemporary Philadelphian was Horatio C. Wood, professor of materia medica and general therapeutics in the University of Pennsylvania's Medical School, who published on myriopods and on arachnids, but not spiders, between 1861 and 1869, before his intensive medical work began. He became a member of our Society in 1866 and died in 1919.

In 1859 the Entomological Society of Philadelphia was founded. None of its original members was a member of our Society. A few years later John Lawrence Le Conte became its President, succeeded by George H. Horn and after the latter's death by Henry C. McCook, well known for his studies of ants and of spiders and who had been elected to the Philosophical Society in 1896.

[24] Henshaw, *Trans. Amer. Ent. Soc.*, **25**: lxvii (1898).

Just as the foundation of the Academy of Natural Sciences of Philadelphia in 1812 drew to it from the Philosophical Society some of the interests centered in natural history or biology, so the formation of the Entomological Society (known since 1867 as The American Entomological Society) has tended, doubtless, to draw from both of the older organizations those attracted to the study of insects. Exact determinations have yet to be made.

On listing the number of papers on terrestrial Arthropods published by our Society between 1768 and 1940, according to the list of 1940, and with 2 additions for 1768, it appears that from 1768 to 1800 the number was 4.

1801–1810...4		1871–1880...21	(maximum)
1811–1820...1	(minimum)	1881–1890... 9	
1821–1830...3		1891–1900...15	
1831–1840...2		1901–1910... 5	
1841–1850...5		1911–1920... 3	
1851–1860...6		1921–1930... 1	(minimum)
1861–1870...3		1931–1940... 3	

The maximum in 1871–80 is largely due to 5 papers by Le Conte and 9 by Horn in that decade.

It is surely desirable that the catholicity of the American Philosophical Society be preserved, among other ways, by the addition of entomological members. Since the death of our distinguished member, William Morton Wheeler, little has been done in that respect, although the names of several investigators in diverse fields of entomology, morphological as well as taxonomic, have been brought to the attention of the Committee on Membership.

I am sensible that I have entirely and unwillingly omitted mention of some other entomologists of our roster previous to 1900. Perhaps my audience is not sorry that I have done so.

THE BEGINNINGS OF VERTEBRATE PALEONTOLOGY IN NORTH AMERICA

GEORGE GAYLORD SIMPSON

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(Read February 14, 1942, in Symposium on the Early History of Science and Learning in America)

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ABSTRACT

The first vertebrate fossils to be seen by Europeans in the Western Hemisphere were mastodon bones collected by the Indians in Tlascala, and shown to Cortez's army in 1519. A few casual finds were made in the next two centuries but these also had no sequel and cannot be called scientific discoveries. The find that may be considered the true discovery in this historical sense was made by a party under the Canadian Charles Le Moyne, second Baron de Longueuil, on the Ohio River in 1739. Mastodon bones were taken to France by Longueuil and studied by Guettard, Daubenton, Buffon, and others. Another important collection was made by Croghan in 1766 and sent to London. It was not until about the end of the eighteenth century that it was firmly established that most of these large bones represented a distinct, extinct, herbivorous species allied to the elephant—the species now called *Mammut americanum*. Recognition of these facts involved the most fundamental principles of vertebrate paleontology. The first American with truly professional competence in research in this field was Caspar Wistar. Early encouragement of the science and of collecting was given by Jefferson. The intimate connection between vertebrate paleontology and public museums began with the Peales, who in 1801 collected and mounted the first assembled fossil skeleton in America and the second in the world. Richard Harlan, in 1823–1843, gave Linnaean names to fossil vertebrates, made a series of studies relatively modern in tone, and was the first American who can be said to have pursued an important and consecutive paleontological career. Less extensive or less important work by his contemporaries is more briefly reviewed. The main center for these early researches was the American Philosophical Society, with which Wistar, Jefferson, the Peales, Harlan, and most others then active in this science were connected. A considerable variety of vertebrate fossils had been found in the far West before 1846, the accepted date of that discovery. Among them were finds by Lewis and Clark in 1804–1806, a good mosasaur skeleton from South Dakota, about 1830, mastodons from western Missouri found in 1806 or earlier, a Pleistocene fauna found in Oregon in 1839, and another found in Alaska in 1816. Scientific publication in America began in the *Transactions* of the American Philosophical Society, which started printing in 1769, and before 1842 numerous scientific periodicals were established and were publishing notices of fossil vertebrates, notably the *American Journal of Science*, established in 1818. 1842 is taken more or less arbitrarily as the end of the pioneer period and the extent of knowledge of American fossil vertebrates at that date is summarized. An appendix gives records of early vertebrate paleontological activities and collections of the American Philosophical Society.

INTRODUCTION

ON April 3, 1789, the Reverend Dr. Nicholas Collin read to the American Philosophical Society a long essay "on those inquiries in Natural Philosophy, which at present are most beneficial to the UNITED STATES OF NORTH AMERICA."[1] "The vast Mahmot [mammoth or mastodon]," he said, "is perhaps yet stalking through the western wilderness; but if he is no more, let us carefully gather his remains, and even try to find a whole skeleton of this giant, to whom the elephant was but a calf." An interest in fossils was not even then new to the Society, or to such illustrious members as Franklin, Jefferson, and Wistar, but the admonition signalized devotion to a subject of inquiry that has been continually before the Society for more than 150 years—indeed, for a longer

[1] Dr. Collin was Rector of the Swedish Churches in Pennsylvania. He was elected a member of the Society on Jan. 16, 1789, and, with what might now be considered brashness, he began his long and active participation by telling the other members in great detail what he thought they should do. The range of Collin's interests was almost as broad, though not as deep, as that of Franklin's and it included, among many other subjects, botanical gardens, lead-glazing, colonial history, the theory of probabilities, philology, climatology, and the invention of a speedy elevator for saving persons and property from the upper stories of a house on fire. Collin died in 1831. For text of his inaugural essay see Gollin, 1793.

time than the science of vertebrate paleontology is commonly supposed to have existed.

The immediate proposal for gathering the whole skeleton of a "mahmot" was soon carried out by the Peales, who assembled and for a time exhibited in the Society's hall a mastodon skeleton. More generally, the whole history of American vertebrate paleontology is intimately connected with the history of the American Philosophical Society.

Following Scott (1927) and Osborn (1931), with considerable modification, the history of this science in America may be divided into six periods:

1. *Pre-scientific Period.*—From the earliest times to about 1762. The first fossil discoveries were made. Toward the end of the period bones were collected and sent to Europe. No truly scientific study of them had been made.

2. *Proto-scientific Period.*—From about 1762 to about 1799. In 1762 Daubenton read a paper on American fossils treating them for the first time in what deserves to be called a scientific way. Vertebrate paleontology was not yet a true science, but basic methods were being invented and sporadically applied. Collections were slowly accumulating.

3. *Pioneer Scientific Period.*—About 1799 to about 1842. In 1799 the first able technical study by an American (Wistar) was published. In Europe this was the epoch of Cuvier, who organized the subject as a true and defined science. Harlan and others in America applied Cuvierian methods and theories to increasingly large collections of fossils. The date 1842 is arbitrary, chosen partly because of the even century since elapsed, but it marked the approximate end of Harlan's career and shortly preceded the beginning of Leidy's.

4. *First Classic Period.*—About 1842 to about 1865. In America the subject was dominated by Leidy, in Europe by Owen. The descriptive science was well established on a comparative anatomical and taxonomic basis, but still lacked the guiding principle of phylogeny. The flood of western discovery began.

5. *Second Classic Period.*—About 1865 to about 1895. Paleontology became evolutionary and developed the theories of phylogeny. Cope and Marsh were the dominant figures in America, although Leidy continued to work until 1890 and numerous other students entered the field. This was a golden age of discovery when most of the major fossil fields of the continent were found.

6. *Modern Period.*—Since about 1895. This has been a period of democratization in which great figures like Scott, Matthew, and Osborn were surrounded by increasing numbers of professional research students and the classic centers of study were supplemented by the rise of many others. Discovery is unabated, but better knowledge of previously known faunas, improvement and diversification of means and subjects of study, and unification and synthesis are characteristic.

It is possible that future historians will find that a sixth period began about 1930 or during the following decade, for vertebrate paleontology seems to be undergoing another radical transformation, but the outcome cannot now be determined and this is, in any case, beyond the scope of the present enquiry.

On the occasion of the 200th anniversary of the formation of the Junto, forerunner of the American Philosophical Society, Professor Scott discussed this history, with special emphasis on the fourth and fifth periods (Scott, 1927). Recently Romer (1941) has summarized the modern period. It would be repetitious and it would invite unfavorable comparison to attempt another account of these parts of the history. There is, however, no adequate historical study of the earliest stages of American vertebrate paleontology, its first three periods as I have listed them, and the present paper is therefore confined to these. The activities of the American Philosophical Society will automatically assume a major part in this study, but their importance is better comprehended and the interests of students are best served, if the whole subject is summarized and the Society's paleontological contributions are placed in this context.

Many individuals and institutions have assisted in the preparation of this paper. It is, of course, based in greatest part on the publications of the period in question and on later published historical studies. In this connection the Osborn Library of Vertebrate Paleontology and the general library of the American Museum of Natural History, the New York Public Library, and the Library of the American Philosophical Society have been used and their librarians have been uniformly helpful. Among them, these libraries have copies of practically every pertinent printed document. Important manuscript materials have been found particularly in the Archives of the American Philosophical Society, the Academy of Natural Sciences of Philadelphia, and the Public Archives of Can-

ada. For information on Longueuil and the first half of the eighteenth century, I am particularly indebted to Mr. Regis Roy, Ottawa, who spent much time searching for published and unpublished information, to Mr. Lawrence J. Burpee, Ottawa, who gave similar assistance and who put me in touch with Mr. Roy, to Mr. A. J. H. Richardson, Public Archives, Ottawa, who made and sent me a copy of an important letter (Wright to Bartram), and to G. Lanctot, Deputy Minister, Public Archives, Ottawa, who provided a photograph of the historic Mandeville-de Lery manuscript map. My original enquiry in Canada was forwarded by Dr. L. S. Russell, Toronto. Dr. W. R. Jillson, Frankfort, Kentucky, provided data on Big Bone Lick, of which he is the leading historian. Mrs. Gertrude Hess, Assistant Librarian of the American Philosophical Society, not only facilitated use of materials in her care but also found, organized, and supplied various important data. Dr. Edwin H. Colbert and Mr. Robert Chaffee facilitated examination of specimens and records in the Academy of Natural Sciences, Philadelphia. Mr. E. T. Hall, Jr., Columbia University, brought to light a remarkably interesting prehistoric fossil discovery. My wife, Dr. Anne Roe, did much of the bibliographic research and assisted in other ways. Several others have given less definite or less extensive aid by making suggestions and comments.

First Glimpses

"I was surprised, a few days since with a present laid before me from Albany by two honest Dutchmen, inhabitants of that city; which was a certain tooth, accompanied with some other pieces of bone; which being but fragments, without any points whereby they might be determined to what animals they did belong, I could make nothing of them."—Governor Dudley, 1706.

Pre-Columbian Indians certainly found and occasionally collected fossil bones, but in general these discoveries are no real part of paleontological history. Various reported Indian legends of fabulous beasts represented by fossil bones have little ethnological and no paleontological value; the data are sparse, often untrustworthy, and carry little conviction of genuine and spontaneous (truly aboriginal) reference to real finds of fossils. Nor is it necessary to examine here the repeated claims that American Indians have left portraits of animals now extinct: when the portraits are unmistakable (like the proboscidean on the Lenape stone), their authenticity and antiquity are more than dubious, and when their authenticity is reasonably established (like the petroglyphs in the Southwest claimed to represent dinosaurs, or the Central American "elephant" carvings), their zoological identification is highly questionable or surely false.

Finds of fossil shark teeth in pre-Columbian sites of the southeastern states and of fossil invertebrates over a wider area (Ball, 1941) prove that even relatively savage tribes did know and pick up fossils. The context of these finds and analogous incidents in historic times suggest that the fossils were "medicine," like many curiously shaped or colored stones, and that there was no particular regard for, probably no knowledge of, their organic origin. Kindle (1935) has devoted a special study to Indian discoveries of vertebrate fossils, but only three instances are circumstantially given and all are relatively late and of little significance. In 1858 an Ojibway told Hind of some bones in western Canada, powdered as medicine by the Indian informant. The bones were not seen, their nature is unknown, and the incident has no real bearing on paleontological discovery. Some time prior to 1853, Indians found mastodon bones on a branch of the Assiniboine and destroyed or hid most of them but took some to Fort Pelly. These were described by Richardson in 1854, but the mastodon was completely known from non-Indian sources long before this. Kindle's third example is erroneous.[2]

After this paper was in manuscript, a discovery of exceptional interest was made by Mr. E. T. Hall, Jr., Department of Anthropology, Columbia University, to whom I am much indebted for this record and for presenting the specimens in question to the American Museum of Natural History. These specimens are undoubtedly the first known fossil vertebrates to be collected by human beings in the Western Hemisphere. They were found one-half mile southeast of Gobernador, northwestern New Mexico, on the floor of a pit house. The associations and conditions of excavation, on the authority of Mr. Hall, are such as to leave no doubt that the fossils were collected and brought into the house by its prehistoric occupants. The

[2] He credits the Indians with discovery of Big Bone Lick because they are said to have guided Croghan there in 1765. In fact the lick and its bones had then been known to whites for more than twenty-five years. As will later be shown, Indians were probably involved in the real discovery of this locality, but even in that instance they cannot fairly be called the discoverers.

date of such occupation is placed somewhere between 700 and 900 A.D., so that these fossils were collected at least 1000 years ago.

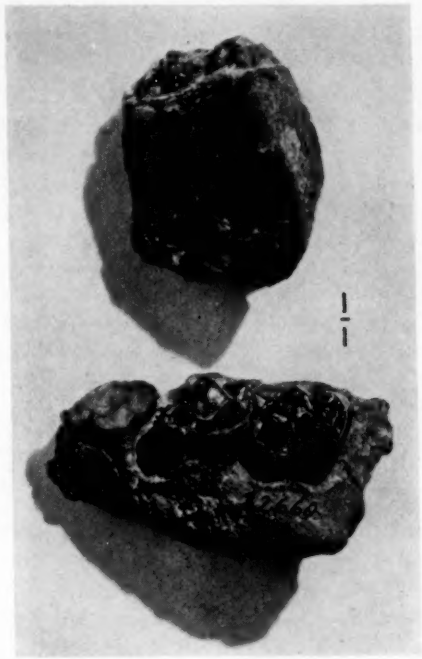

FIG. 1. The first vertebrate fossils collected in the Western Hemisphere. These specimens were collected by Indians in Lower Eocene beds of northwestern New Mexico over 1000 years ago (700–900 A.D.) and taken by them to a pit house from the ruins of which they were excavated in 1941 by E. T. Hall, Jr. The upper specimen, Amer. Mus. No. 32661, is a fragment of lower jaw of an unidentified carnivore. It was ground smooth on one end (right in the photograph) by its prehistoric owners. The lower specimen, Amer. Mus. No. 32660, is part of an upper jaw of *Phenacodus primaevus*.

The two specimens (Fig. 1) are fossil mammals. One is a fragment of upper jaw with three teeth, indistinguishable from *Phenacodus primaevus*. The specimen unquestionably came from the Lower Eocene, so-called Wasatch, properly Largo and Almagre, beds that outcrop widely near the locality of the pit house. These formations and their important mammalian fossils were discovered (in a scientific sense) by E. D. Cope in 1874, some ten centuries after their Indian discovery. The *Phenacodus* jaw shows no evidence of human action, other than its transportation to the pit house, and was apparently retained as found, as a curiosity probably with imputed magical properties. The second specimen is a fragment of lower jaw without tooth crowns and unidentifiable except that it probably belongs to one of the larger carnivores of the early Eocene. One end has clearly been ground down by the Indian owner, as can be seen in the photograph (Fig. 1), and there seems also to have been some grinding, but less deeply, on the side. There are remnants of a concretionary hematite coating, which would make the powder pink or red, and it could have been used as paint. (There is no hematite on the *Phenacodus* specimen and it could have served no directly utilitarian purpose.)

Another, much later Indian discovery that is at the same time the first notice by Europeans of fossil bones in the Western Hemisphere has been generally overlooked by paleontological historians. Writing in 1568 about events that happened in the latter part of 1519, Bernal Diaz del Castillo, a captain in the army of Cortez, tells of great bones in Tlascala.[3] "[The Tlascalans] said that their ancestors had told them, that in former times the country was inhabited by men and women of great stature, and wicked manners, whom their ancestors had at length extirpated; and in order that we might judge of the bulk of these people, they brought us a bone which had belonged to one of them, so large, that when placed upright it was as high as a middling sized man; it was the bone between the knee and the hip; I stood by it, and it was of my height, though I am as tall as the generality of men.[4] They brought also pieces of other bones of great size, but much consumed by time; but the one I have mentioned was entire; we were astonished at these remains, and thought that they certainly demonstrated the former existence of giants. This bone was sent to Castille for his Majesty's inspection, by the first persons who went on our affairs from hence."

The reaction of the King, who had asked for gold and was sent a bone, is not on record, and I have not learned anything further of this first fossil sent from the New World. There can be little doubt that it was a fossil, probably some kind of mastodon.[5] The Indian interpretation of the

[3] I am indebted to Dr. E. H. Colbert for calling my attention to this incident, which I, too, had overlooked on first writing this paper.

[4] A fossil femur found in this region cannot have exceeded five feet in length. The conquistadores were small men by our standards, but not this small. The account was written fifty years after the event, and even at the time some degree of exaggeration would be excusable (G. G. S., note).

[5] Dr. G. L. Jepsen (pers. com.) points out that mastodon remains are still abundant in the vicinity of Tlascala and that local collections of the twentieth century parallel that

bones as those of human giants was at that time (and for long after) the orthodox belief regarding such discoveries made in Europe.

The Jesuit missionary d'Acosta made similar observations in South America later in the 16th century and interpreted them in the same way, but in North America the observations of the doughty Bernal Diaz were followed by a long hiatus. The silence on this subject of the earlier English and French explorers, despite their interest in the animals and other productions of the New World, is indirect evidence that the abundant occurrence of fossil bones in North America was not widely known among the Indians and not a common subject of remark by them.

In 1636 Samuel Maverick reported at Boston that buried shells and bones, including whale bones, had been found about sixty miles above the mouth of the James River in Virginia (Neill's *Virginia Carolorum*, p. 131, reported in Goode, 1901, p. 430). Although fossils in the elementary sense of having been buried, there is no evidence that these bones belonged to an extinct animal. Fifty years after Maverick, in 1686, John Banister is said to have had in his possession and to have exhibited to an English traveler large bones and teeth from the interior of Virginia.[6] Corroborative detail is lacking and again the find led to no scientific result, but it is possible that this indicates the first discovery of true remains of extinct animals in what is now the United States. Banister, a clergyman of the Church of England, emigrated to Virginia prior to 1668 and was one of the most active 17th century colonial students of natural history, especially botany. He may be called our first scientific martyr, having died in 1692 as a result of a fall suffered while botanizing on the Roanoke.

In 1743 Catesby reported finds that may surely be recognized as of extinct animals. This publication was subsequent to the definitive discovery by Longueuil, soon to be discussed, but it probably refers to finds made at considerably earlier dates and is in any case among the first publications that give a definite indication of what animals are concerned. Catesby's remarks are as follows (1743, Vol. II, appendix, p. vii):

> All parts of *Virginia*, at the Distance of Sixty Miles, or more, ["from the sea," evidently omitted] abound in Fossil Shells of various Kinds, which in *Stratums* lie imbedded a great Depth in the Earth, in the Banks of Rivers and other Places, among which are frequently found the *Vertibras*, and other Bones of Sea Animals. At a place in *Carolina* called *Stono*, was dug out of the Earth three or four Teeth of a large Animal, which, by the concurring Opinion of all the *Negroes*, native *Africans*, that saw them, were the Grinders of an Elephant, and in my Opinion they could be no other; I having seen some of the like that are brought from *Africa*.

Whatever the "Vertibras" may have been, the elephant grinders were evidently from one of the southern mammoths, which were, in fact, elephants and which somewhat more closely resembled the living African species than did the Siberian mammoths. If, as is probable, this incident preceded 1739, and because identifications of still earlier date speak of human giants or of bones questionably fossil, it appears that these negro slaves made the first technical identification of an American fossil vertebrate—a lowly beginning for a pursuit that was to be graced by some of the most eminent men in American and in scientific history.

Whatever the true status of the discoveries connected with Maverick and with Banister, and whatever the dates of those mentioned by Catesby, fossil mammals were certainly known in the English colonies early in the 18th century. Under the date "Roxbury, 10 July, 1706," Governor J. Dudley of Massachusetts wrote to the eminent divine Dr. Cotton Mather and told him of finds of enormous teeth and bones made along the Hudson River during and about 1705 (Dudley's letter in Warren, 1855, pp. 196–197). "I am perfectly of the opinion," wrote the governor, "that the tooth will agree only to the human body, for whom the flood only could prepare a funeral. . . . There is nothing left but to . . . allow Dr. Burnett and Dr. Whiston to bury him at the Deluge; and, if he were what he shows, he will be seen again at or

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of the Indians in the sixteenth. *Rhynchotherium tlascalae* Osborn (type of this important genus) is based on a lower jaw from this region, a cast of which was preserved in Geneva at least as early as 1856.

It is unlikely that such striking objects escaped the attention of the early natural historians of the Spanish possessions, who did give notices of fossil bones in South America, at least. A search of that literature may produce interesting data also on Mexican discoveries, but limitations of space and time have prevented me from undertaking so considerable a task regarding a topic that is only incidental to my main theme.

[6] The incident is often mentioned, but without reference to a contemporary source. I cite it from Goode (1901, p. 385), who seems also to be the authority for the remarks of Osborn (1931) and of others who mention the incident in historical notes.

after the conflagration, further to be examined."

Fortified with this information (the source of which he did not state), Mather wrote on Nov. 17, 1712, to Dr. John Woodward of London. He told of a large manuscript, two volumes in folio, designed to illustrate and to comment on the Bible by observations on the natural history of America. The author of this *Biblia Americana*, as he called it, was not named but was perhaps Mather himself. He writes to recommend it "to the Patronage of some generous Mecaenas, to promote the publication of." As a specimen of the manuscript, a note is given on the passage in Genesis, Chapter 6, Verse 4, relating to giants. The note cites the occurrence of gigantic teeth and bones judged to be human, particularly a tooth, four pounds and three quarters in weight and a thigh-bone, seventeen feet long, brought from Albany to New York in 1705. Another tooth and some bones were found at "Cluverack," thirty miles from Albany. "He then gives the Description of one, which he resembles to the Eye-Tooth of a Man; he says it has four Prongs, or Roots, flat, and something worn on the top; it was six inches high, lacking one eighth, as it stood upright on its Root, and almost thirteen inches in circumference; it weigh'd two pounds four ounces *Troy* weight." Another tooth was found far beneath the surface of the earth in the bank of the Hudson River about fifty leagues from the sea.

We may still ardently agree with the secretary who caused this communication to be inserted in the *Philosophical Transactions* (see Mather, 1714) that "it were to be wish'd the Writer had given an exact Figure of these Teeth and Bones." There is, however, no serious doubt that they were mastodon remains. The abstract of Mather's letter was perhaps the first printed notice of the occurrence of such fossils in North America. Mather and his informant Dudley were still medieval in their interpretation of the bones. The next finds, that of Longueuil in 1739 (see below) and that published by Catesby in 1743, were immediately recognized as belonging to animals of the elephant kind, and the first turning point in paleontological history had been reached.[7]

[7] One other early glimpse may be mentioned although my data on it are insufficient. Smallwood and Smallwood (1941, p. 88) mention a manuscript letter from Joseph Lord to James Petiver, Sept. 1, 1707, in the Sloane Collection of the British Museum. In this letter Lord told of the tail of a small fish found in a flat and broad piece of stone. The Smallwoods do not quote the data as to locality that were presumably given in the original letter, and the manuscript is not now available to me. The incident is nevertheless worthy of notice because it is unique for its date, and for a long time after, in involving a *small* fossil vertebrate. Most of the eighteenth century naturalists overlooked bones of animals smaller than the mastodon, although they were quite familiar with small fossil shells.

Longueuil, 1739, to Croghan, 1766

"They are extremely curious on many accounts; no living elephants having been seen in any part of America by any of the Europeans settled there, or remembered in any tradition of the Indians."—Benjamin Franklin, 1767.

If Columbus discovered America in 1492, Charles Le Moyne, second Baron de Longueuil, discovered American fossil vertebrates in 1739. Both had predecessors, and those of Longueuil have been discussed, but in neither case did the predecessors initiate permanent record or continuous development. Not forgetting the pioneers who saw, marveled, and passed on without sequel, the scientific historian may nevertheless define true discovery, for his purposes, as that leading by a traceable route, however devious, to eventual elucidation of the problems concerned.

In the course of the present investigation so much confusion regarding Longueuil's discovery was found and such a large amount of evidence concerning it was brought together that a separate paper has been devoted to this and ancillary aspects of paleontological history in America (Simpson, 1942). Previous contradictions and doubtful points have there been fully exposed, the circumstances surrounding the events have been summarized, and the available data have been listed and considered in detail. In the present paper, therefore, only a brief account of the conclusions reached will be given, referring the reader to the more special paper for most of the corroboration, qualification, and citation.

In 1739 Longueuil was placed in command of French and Indian troups despatched from Canada to aid Le Moyne de Bienville, founder and governor of New Orleans, in an attack on the Chickasaw Indians. Longueuil (born 1687, died 1755), son of a distinguished Canadian family, was then a major in the French colonial army and was destined to occupy the important post of Governor of Montreal, 1749–1755. The expedition left Montreal in June, 1739, and proceeded to the Ohio River by way of Oswego, Lake Chautauqua, and the Allegheny River. In late summer of that year they descended the Ohio and at some distance be-

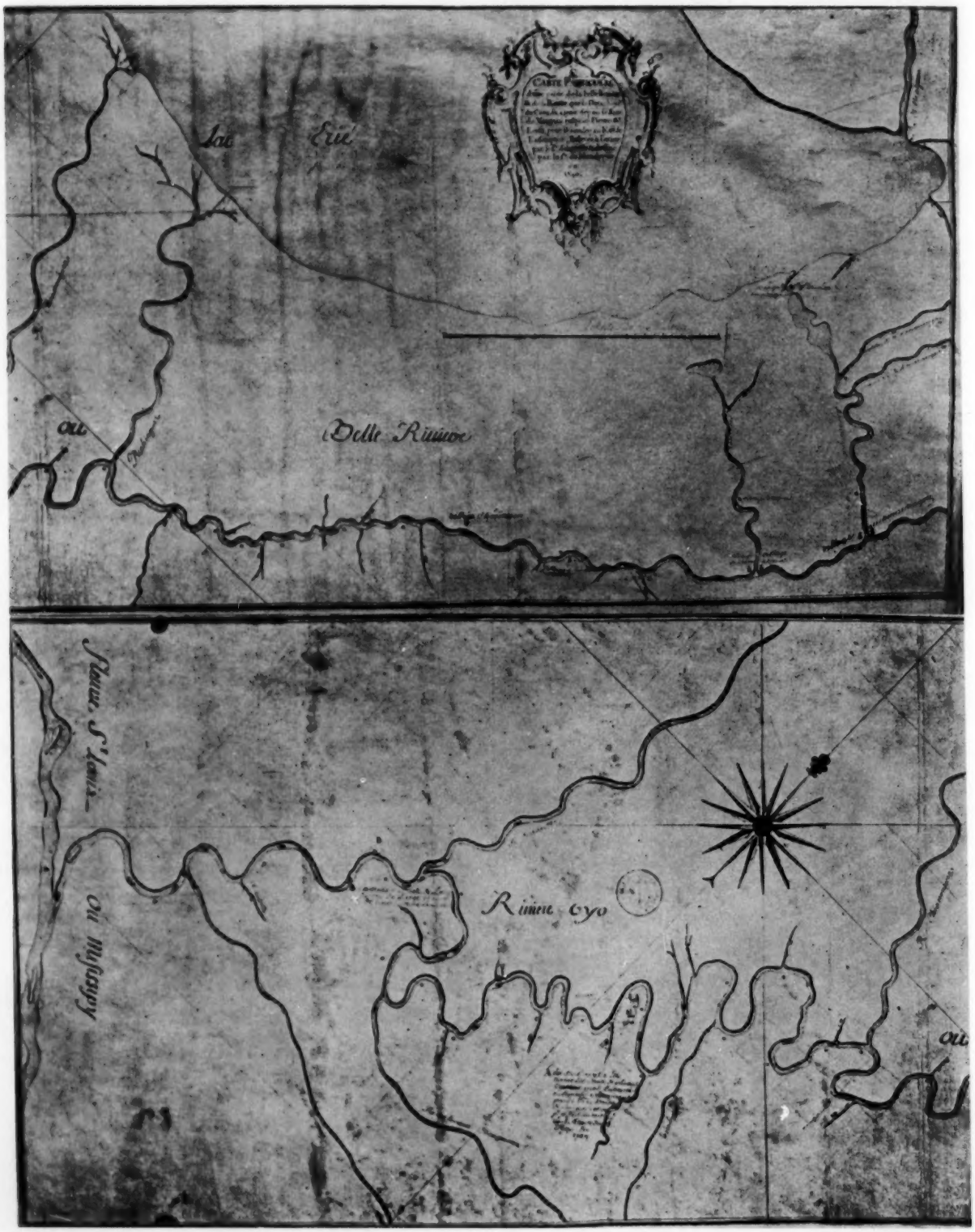

FIG. 2. Map of the Ohio River, drawn in 1740 by Mandeville from an approximate traverse by de Lery in 1739. The site of the discovery of fossil bones by Longueuil in 1739 is shown (see Fig. 3). The manuscript map has been photographed in two overlapping sections. Original in the "Depot des Cartes et Plans de la Marine, Service Hydrographique," Paris. Photographic copy from the Public Archives of Canada.

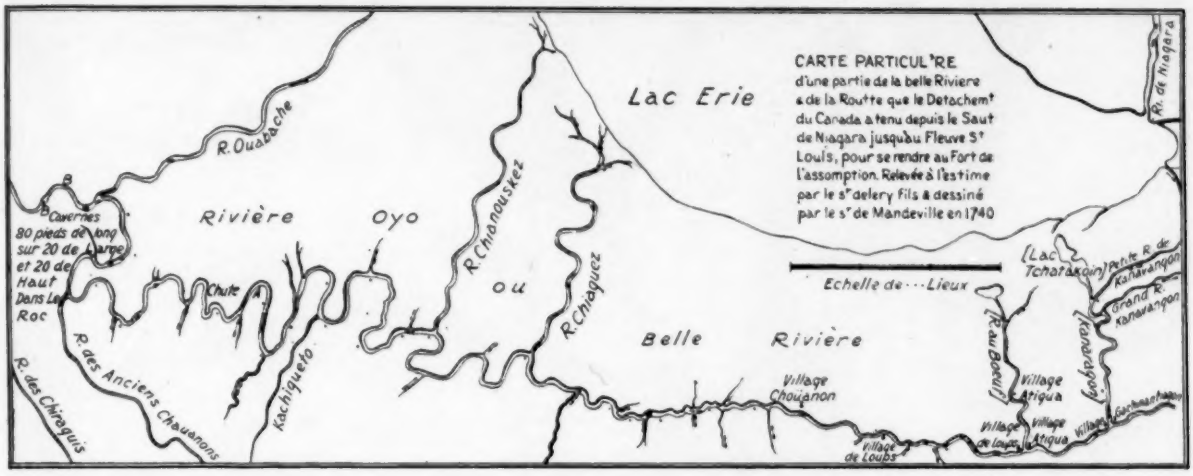

FIG. 3. Drawing of part of the map shown in Fig. 1, to reproduce some of its details more clearly. *A* is the fossil locality. The three names in brackets are illegible on the copy of the map available to me and are supplied from other early French sources. The spelling of some of the other names is uncertain but is about as shown.

fore reaching the falls, where Louisville now stands, they found a marsh on the edge of which were large bones and teeth, representing what they took to be the remains of three elephants. Longueuil had some of these remains gathered up, including a tusk, a femur, and at least three molars, and these were carried with the army to its rendezvous with Bienville, on the Mississippi near the present site of Memphis.

After the successful conclusion of the Chickasaw war in the spring of 1740, Longueuil went on to New Orleans, taking the fossils with him, and hence transported them to France at about the end of 1740. The fossils were placed in the King's collection of curiosities, Cabinet du Roi, whence they were transferred to the natural history museum in the Jardin des Plantes, where Cuvier reported their continued preservation some seventy-five years later and where they may still be. This collection of fossil bones, the first from America ever to receive definitely scientific attention, was studied by Guettard, Daubenton, Buffon, and others, as will be related in the next section of this paper.

Longueuil's fossil locality was noted by Chaussegros de Lery, fils, a young (18-year-old) engineer who accompanied the expedition, and it was recorded on a manuscript map (Figs. 2–3) drawn up from de Lery's data in 1740 by Philippe Mandeville, sieur de Larigny, a lieutenant with the Louisiana contingent. From this map the indication of the fossil locality was copied, with less precision and with a serious error, 1729 for 1739, by Nicolas Bellin on his map of Louisiana and neighboring regions published in Paris in 1744 (Fig. 4).

Such students as have been aware of Longueuil's discovery have assumed that it was made at the locality later famous as Big Bone Lick, in what is now Boone County, Kentucky, southwest of Covington. Examination of the Mandeville-de Lery map casts some doubt on this conclusion. On this, the only contemporary source that survives, Longueuil's' locality seems to be distinctly farther down the Ohio, nearer Louisville, than is

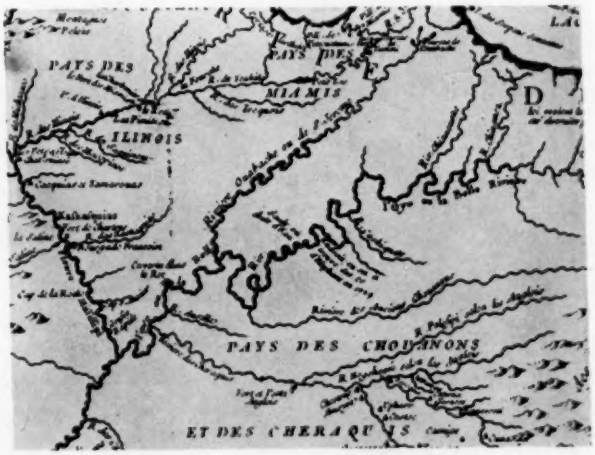

FIG. 4. Part of the map of Louisiana, etc., published by Bellin in 1744. The course of the Ohio River is based on the manuscript map of Fig. 2, and the Longueuil fossil site is designated in a more general way and with the erroneous date "1729" for "1739." (Photostat of the New York Public Library copy.)

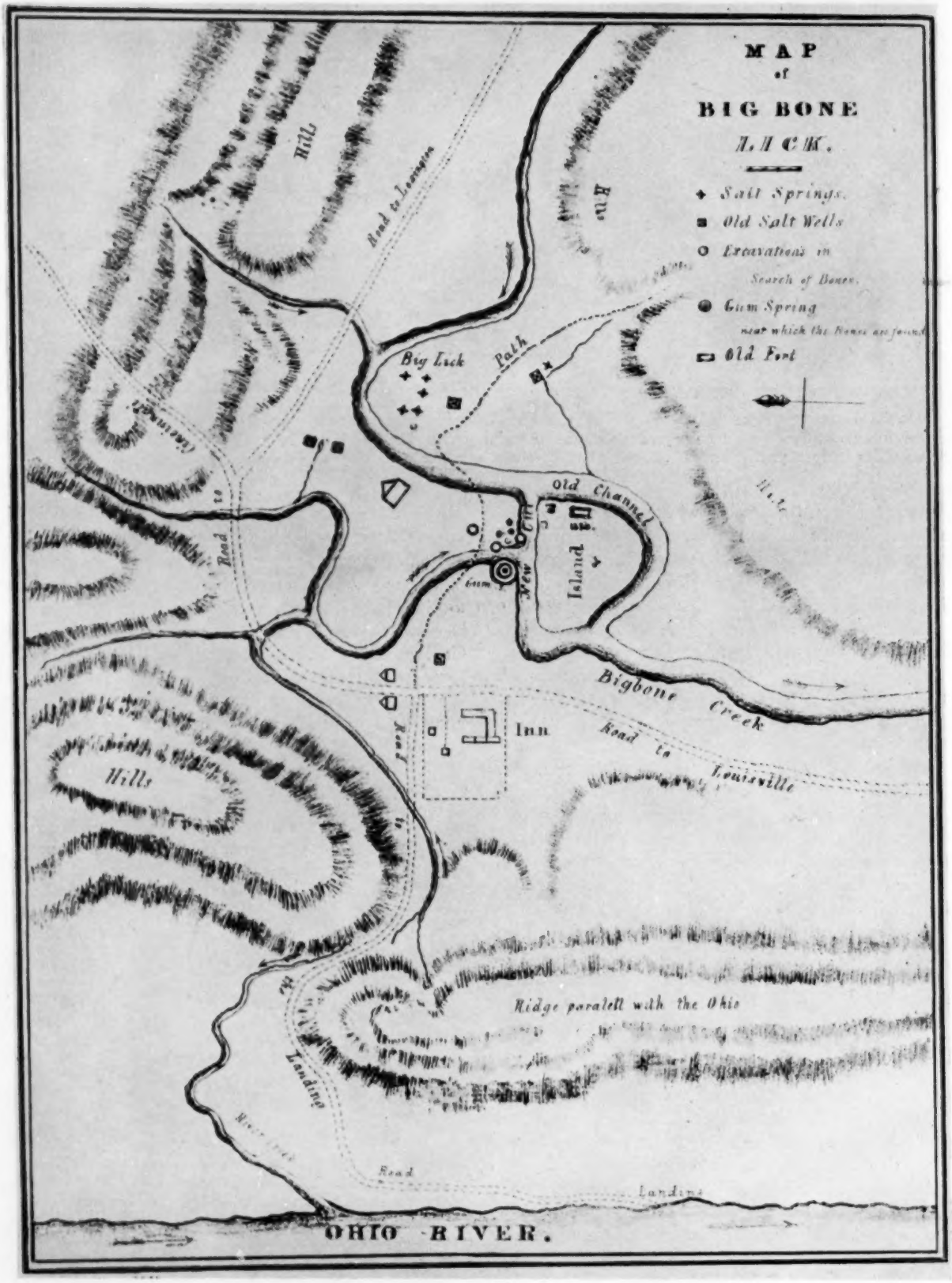

FIG. 5. Map of Big Bone Lick, most famous of early American fossil localities, as it was about 1828. Drawn by I. Cozzens from data of William Cooper. From Cooper, 1831.

Big Bone Lick, even with due allowance for the inaccuracy of this hurried and rough survey. At present it cannot be affirmed that Longueuil's locality is known or that it was, or was not, Big Bone Lick. It was, however, on the south (now the Kentucky) side of the Ohio between the present cities of Covington and Louisville.[8]

In 1751 Christopher Gist passed near and may have visited Big Bone Lick.[9] On March 13 of that year, while in this vicinity, he was given two teeth of a large beast (doubtless a mastodon) found at or near that locality. One Robert Smith, residing in the region, was then familiar with the occurrence of big bones and teeth thereabouts and told Gist that they had been discovered some seven years earlier, *i.e.* about 1744. Whether this was an inaccurate memory of Longueuil's discovery or refers to a later, independent discovery by English colonial pioneers is not clear. The importance of the incident is that it shows that English settlers or traders were then in this vicinity and that they knew of the fossil deposits. Most of these pioneers were driven out by the French in 1752–1754, but they were spreading down the Ohio Valley again in the 1760's and definitively occupied it in the 1770's.

FIG. 6. Big Bone Lick today. View looking down the valley, toward the Ohio. Most of the bones were found in the alluvial floor of the valley toward the center and to the right in the photograph. Photographed by the author in 1941.

Gist gave one of his fossil teeth to the Ohio Company and it may have been one of those that later turned up in London without definite record, but its fate is not known.

It is probable that numerous whites, British or French, visited Big Bone Lick during the later 1750's, as suggested by a few records such as those of John Findley in 1752 and of Mary Inglis, with French and Indian captors, in 1756 (see Jillson, 1936). These visits have no bearing on paleontological history except to suggest increasing familiarity with the locality among the usually inarticulate frequenters of the disputed wilderness.

Word of the great bones early reached John Bartram, probably the most famous of American

FIG. 7. One of the salt springs of Big Bone Lick. This spring, kept open by an old, large, iron pipe, is still flowing. It is directly below the present small settlement of Big Bone. Photographed by the author in 1941.

[8] Present-day readers of the eighteenth and early nineteenth century literature are often confused by references to "Canadian" discoveries, to the "Ohio" animal or animal from Ohio, to fossils from western Virginia, etc. These usually relate to remains found in what is now Kentucky. Longueuil planted the French flag at his fossil locality and claimed it for Canada. His specimens and others from the same region were Canadian to the French students and to others deriving their information from French sources, even as late as the end of the eighteenth century, long after France and Canada had relinquished all claim to this area. To the English, this region was in Virginia even before force of arms had rebuffed the French claim. As far as the English colonies were concerned, what is now Kentucky was in Augusta County, Va., in 1738–1769, in Botetourt County in 1769–1772, and in Fincastle County in 1772–1776. In 1776 Kentucky, under that name and with more or less its present boundaries, became a county of Virginia, and in 1792 Kentucky joined the union as a separate state. Big Bone Lick and other Kentucky localities have never been in Ohio, but the name of the river used in designating the region has occasionally been mistaken for that of the state.

[9] Jillson (1936), who has given the most detailed and in general much the best account of early visits to the Kentucky fossil fields, maintains that Gist did visit this famous lick. In this he contradicts the conclusion of Gist's editor, Darlington. I have discussed this relatively minor difference of opinion elsewhere (Simpson, 1942).

Colonial naturalists, who was ever eager for any intelligence regarding the natural productions of the colonies and their frontier. He sought information from his correspondent James Wright and received it abundantly in the following letter:[10]

Respected friend

Pursuant to thy request, I have made as particular an Enquiry relating to those bones thou mentions, as I possibly Could, from two Sencible Shawanese [Shawnee] Indians, Assisted by an Interpreter, And the Substance of what they Say is as follows—the place where they lye is about 3 miles from the Ohio, salt & moist, as well as I could judge by their description of it seems to contain 30 or 40 Acres, in the Midst of a large Savannah, 4 days Journey Below the lower Shawanese town, on the East Side of the river, that there appear to be the remains of 5 Entire Sceletons,. with their heads All Pointing towards Each other, And near together, supposd to have fallen at the same time; when they were desired to describe their several parts, they began with their heads, of which two were larger than the rest, one of these, they said a Man Could but Just Grasp in Both his Arms, with a long Nose, And the Mouth on the under side, they next mentiond the shoulder blade, which when set on End, reached to their Shoulders, And they were both tall men, What they Call'd the Cup (or socket) of this bone, was equal in size to a large bowl, the thigh bone when broke assunder, would admit of a little boy's Creeping into it—they were askd if they had seen those long bones they Call'd horns, they Answered they had, And by the distance from where they stood to the door, Showd them to be 10 or 12 feet long, And added that by the Bones, they Judged the Creature when Alive must have been the Size of a Small house, pointing from the Window to a Stable in Sight;—I askd them if the Place where they lay was Surounded with Mountains, So as to admit a probability of its Ever having been a lake, they Answered, the place was salt and Wettish, And by having been much trod & Licked, was somthing lower then the adjacent land, which however, was so level, to a prodigious Extent, that the lick, as they Calld it, Could never have been coverd with water; And that there were many roads thro this Extent of land, larger & more beaten by Buffalas and other Creatures, that had made them to go to it, than any Roads they saw in this Part of the Country on being Questiond if they had seen such bones in Any other place, they said they had seen many such, Scattred here & there in that large tract of land mentiond before, some upon the Surface, and some Partly burned [buried?], but all much more decay'd by time, then those they had been describing, and not Any Entire Sceleton; I Askd if they had Ever heard from their old men, when these 5 were first observed, or if they, or their fathers, had Ever seen any such large Creatures living, as these bones were supposd to have been a part of, they Answered they had never heard them spoken of, other then as in the Condition they are at present, nor ever heard of any such creature having been seen by the oldest Man, or his father—that they had indeed a tradition, such mighty Creatures, once frequented those Savannahs, that there were then men of a size proportionable to them, who used to kill them, and tye them in Their Noppusses And throw them upon their Backs As an Indian now dos a Deer, that they had seen Marks in rocks, which tradition said, were made by these Great & Strong Men, when they sate down with their Burthens, such as a Man makes by sitting down on the Snow, that when there were no more of these strong Men left alive, God Kiled these Mighty Creatures, that they should not hurt the Present race of Indians, And added, God had Kill'd these last 5 they had been questioned about, which the Interpreter said was to be understood, they supposed them to have been Killd by lightning—these the Shawanese said were their traditions, and as to what they knew, they had told it—the Man who Interpreted, was well Acquainted with their language, and as I have known him from a boy, I am Confident he would do it faithfully, I shal be pleas'd if what smal Information I have gain'd wil be agreable to thee, And shal be glad to oblige thee at any time to the Utmost of my Power,

I am thy Assured friend

James Wright

August 22nd 1762
John Bartram

There is, as usual in the earlier records, some doubt as to the locality here described but it may have been Big Bone Lick. This is indeed in a valley surrounded by wooded hills and not in "a large Savannah," but reference is probably to the more distantly surrounding level uplands. This lick could well be described as a salt and moist tract of 30 or 40 acres, something lower than the adjacent land, about 3 miles from the Ohio River, and about 4 days' journey below the lower Shawnee town, which was near the junction of the Scioto and the Ohio (A. J. H. Richardson, pers. com.). The report that bones were widely scattered over a large tract in this region is important in view of the tendency to ascribe all early finds along the Ohio to the single locality of Big Bone Lick, itself.

[10] The original is British Museum, Additional Mss. 21648, ff. 333–4. It is copied in Public Archives of Canada, Bouquet Papers, Vol. 10–2, pp. 279–82, from which Mr. A. J. H. Richardson kindly provided a transcript for my use.

John Bartram (1699–1777) was an "Old Member under the Original Plan" of the American Philosophical Society and it is highly probable that the information thus obtained was communicated to the Society, although the incident antedates any surviving proceedings. The paleontological interests of members of the Society may certainly, and those of the Society itself may with sufficient probability, be dated from 1762, at latest.[11]

The most famous collection of fossils made in the Ohio Valley during the eighteenth century was Croghan's, but others were made and sent to Europe between Longueuil's and Croghan's by collectors whom the accidents of time have left anonymous, as far as published record appears. Thus we learn from Collinson (1768a) that a Captain Owry of Hammersmith, who had served in America, had, sometime previous to 1767, acquired there a "calf elephant tusk" and "a great pronged tooth" (both obviously of the mastodon). Earl Bute also then had in his possession at least one molar (Collinson, 1768b). Moreover, in the spring of 1767 before Croghan's collection reached England, Hunter (1769) found in the Tower, in charge of Mr. Bodington, "a considerable quantity of elephants [mastodon] teeth [that] had been brought . . . from America." Unfortunately Hunter neglected to record the "verbal account of their having been brought from the banks of the Ohio" as it was given to him by Mr. Bodington.

Croghan's activities have been recounted by Kindle (1931) in an admirable paper that has as its only defects the claim that Croghan discovered Big Bone Lick and that he was the first collector of Kentucky fossils. It has already been made sufficiently clear that Croghan has no title to either of these honors, but it may still be maintained that he was the most important 18th century American collector whose name and achievements are known to us. For this reason and because of the connection with Benjamin Franklin, whose interests are so dear to the American Philosophical Society, some repetition of Kindle may be risked by treating the incident at sufficient length.[12]

Born in Dublin, Ireland, of Protestant parents at an unknown date early in the eighteenth century, Croghan migrated to Pennsylvania in 1741. He soon became familiar with the Delaware and Iroquois Indians, whose languages he learned, and he established trading posts in the upper Ohio Valley. His men were driven out of the region by the French in 1752–54 and he became an active partisan of and participant in the British advances that ultimately stifled French pretensions here. In 1756 he was made deputy superintendent of Indian affairs, with onerous duties that he performed with remarkable skill, and in 1758 he assisted in the capture of Fort Duquesne. He then moved to that site (Pittsburgh), built "Croghan Hall," acquired a great estate, and engaged in extensive land speculations, often in competition with George Washington. At the outbreak of the revolution he was unjustly considered a Tory, lost all his property, and died in poverty on Aug. 31, 1782. His biographer, Volwiler, considers that, "Next to Sir William Johnson, Croghan was the most prominent Indian agent of his time. . . . He was one of the first Englishmen [*sic!*] to foresee the future greatness of the wilderness beyond the Appalachians."

In 1765 Croghan was sent to explore the region of the Ohio, to treat with the Indians who retained French sympathies, and to attempt to open the Illinois region to British penetration. He left Fort Pitt on May 15, 1765, and on the 30th of that month he came to Big Bone Lick which he described as follows in his diary (diary in full in Featherstonhaugh, 1831; pertinent passages copied in Kindle, 1931; Jillson, 1936):

> We passed the great Miame River, about thirty miles from the little river of that name, and in the evening arrived at the place where the elephants' bones are found, where we encamped. . . . This day we came about seventy miles [hence about forty beyond the Miami].
>
> 31st. Early in the morning we went to the great Lick, where those bones are only found, about four miles from the river, on the south-east side. In our way we passed through a fine timbered clear wood; we came into a large road [game trail] which the buffaloes have beaten, spacious enough for two wag-

[11] The James Wright in question, clearly a Quaker (as was Bartram), is doubtless the James Wright of Delaware County (Pennsylvania) who was elected a member of the Society in 1768.

In 1804 Benjamin Smith Barton published a close paraphrase of this letter, from a manuscript (clearly not the letter itself) in his possession. The letter writer was referred to as "James" in this manuscript and Barton erroneously identified him as James Logan. The only additional information given by Barton is that Bartram's interest arose from his being informed by Colonel Bouquette that Shawnee Indians had brought to Pittsburgh a large tooth and part of a tusk.

[12] The data, however, are from Kindle's sources, and others, and not paraphrased from Kindle. On Croghan's life, aside from his fossil collecting, see Volwiler, 1930, and his references.

ons to go abreast, and leading straight into the Lick. It appears that there are vast quantities of these bones lying five or six feet under ground, which we discovered in the bank, at the edge of the Lick. We found here two tusks above six feet long; we carried one, with some other bones, to our boats, and set off. . . .

On June 7th, while encamped a short distance below the mouth of the Wabash, Croghan's party was attacked by hostile Indians, who killed five men and took the others captive. "I got the stroke of a Hatchet on the Head," Croghan later wrote to his friend Captain Murray (Volwiler, 1930), "but my skull being pretty thick, the hatchet would not enter, so you may see a thick skull is of service on some occasions." It need hardly be added that the fossils were abandoned.

Croghan eventually obtained his freedom and after weary peregrinations found his way back to Fort Pitt. Nothing daunted, he set out for Illinois again on June 18, 1766, in company with Captain Harry Gordon and an escort. On July 17 they visited Big Bone Lick, of which Gordon wrote (Public Archives of Canada, Shelborne Mss., Vol. 48, pp. 159–178, pertinent passages published by Kindle, 1931, from a copy by J. Coussens): "The extent of the Muddy part of the Lick is ¾ of an Acre; this Mud being of a salt quality is greedily lick'd by Buffalo, Elk & Deer, who came from distant parts, in great Numbers for this Purpose; we picked up several of the Bones, some out of ye Mud, others off the firm Ground. . . ."

Croghan went on to Illinois, where malaria cut short his stay, and then to New Orleans and New York. Early in 1767 he dispatched the Big Bone Lick fossils to London, part to Lord Shelburne, then in charge of American colonies and later Prime Minister, and part to Benjamin Franklin.

As listed by Collinson (1768a) the Croghan collection sent to Lord Shelburne included two tusks, several molar teeth, and a lower jaw with two teeth. Most of these were deposited in the British Museum. Franklin's share included four tusks, another broken tusk and still another much decayed, a vertebra, and three molars. Franklin acknowledged the receipt of these important relics in the following letter:[13]

To George Croghan

London, Aug. 5, 1767

Sir,

I return you many thanks for the box of elephants' tusks and grinders. They are extremely curious on many accounts; no living elephants having been seen in any part of America by any of the Europeans settled there, or remembered in any tradition of the Indians. It is also puzzling to conceive what should have brought so many of them to die on the same spot; and that no such remains should be found in any other part of the continent, except in that very distant county, Peru, from whence some grinders of the same kind, formerly brought, are now in the museum of the Royal Society. The tusks agree with those of the African and Asiatic elephant in being nearly of the same form and texture, and some of them, notwithstanding the length of time they must have lain, being still good ivory. But the grinders differ, being full of knobs, like the grinders of a carnivorious animal; when those of the elephant, who eats only vegetables, are almost smooth. But then we know of no other animal with tusks like an elephant, to whom such grinders might belong.

It is remarkable, that elephants now inhabit naturally only hot countries where there is no winter, and yet these remains are found in a winter country; and it is no uncommon thing to find elephants' tusks in Siberia, in great quantities, when their rivers overflow, and wash away the earth, though Siberia is still more a wintry country than that on the Ohio; which looks as if the earth had anciently been in another position, and the climates differently placed from what they are at present.

With great regard, I am, Sir,

Your most obedient humble servant,

B. Franklin

A subsequent letter to the Abbé Chappe (see next section of this paper) shows that Franklin soon changed his mind as to the habits of the animal from the Ohio.

By 1767 fossil collectors in the New World had provided the savants of the Old World with ample materials for astonishment and reflection. The interpretation of these remains was the first task of American paleontology.

Identifying the Vast Mahmot

"We are forced to submit to concurring facts as the voice of God—the bones exist—the animals do not!"—Rembrandt Peale, 1803.

It took almost the whole eighteenth century to answer a few questions regarding remains like those reported by Mather at the beginning of that

[13] The original has not been located, but the letter is printed in most editions of Franklin's works. It is here quoted from the Smyth edition. The Jared Sparks edition of 1838 includes a helpful note with a quotation from Collinson.

century. Mather had replied correctly only to the most fundamental and simple of such queries: he accepted the obvious fact that these fossils are of organic origin.

From what sort of creatures did the bones and teeth come? Mather said from humans. His first successors correctly said from lower animals. Enlarging knowledge then broke the question into several. What general sort of animals? This was already solved by Catesby's slaves and by Longueuil's companions: elephant-like animals. Were they distinct from living elephants? We will find Collinson replying affirmatively for some of the remains and his early successors answering correctly for all. Were they the same as the Siberian mammoth? Answer was long confused by lack of appreciation that more than one kind of animal was concerned, leading to the next question: Were two or more distinct American animals confused under the name mammoth? No one clearly saw the possibility until late in the 1790's, after which a correct conclusion was soon reached.

Were these animals extinct? Hunter replied correctly in 1768, but the argument continued until the end of the century.

What were their habits? Collinson had the an-

CARTE MINÉRALOGIQUE,
Où l'on voit la Nature des Terreins du CANADA et de la LOUISIANE.
Dressée par Philippe Buache de l'Ac. des Sciences sur les recherches et pour un Mémoire de Mr Guettard de la même Acad.
1752.
Explication des Caractères.
Baye de Baffins
Baye d'Hudson
Détroit de Davis
Détroit d'Hudson
Labrador
Terre Neuve
Mer de l'Ouest
Gr.de Eau
Quivira
Panis
Sioux
Illinois
LOUISIANE
OCÉAN SEPTENTRIONAL
Détail particulier des Rivages du Fl. St. Laurent depuis la Malbaye jusqu'à Montreal.

FIG. 8. Mineralogical map of North America by Philippe Buache from the data of Guettard, from Guettard, 1756. In this, probably the first geological map of the continent, several invertebrate fossil localities are marked and the vicinity of the Big Bone Lick (south of the "R. d'Ouabache") is shown but not marked.

swer in 1767, but again it took the rest of the century for agreement to be reached.

How could such tropical animals as elephants live in the cold climate of Ohio and still farther north? Franklin gave an intelligent but wrong answer in 1767, and Jefferson gave an essentially correct answer in 1781 or 1782, but new discoveries seemed for a time to contradict him and it was years before the solution was clearly understood and generally accepted.

These questions all seem very simple now. We might think that the first student to bring together some of the American fossils, some from Siberia, and recent elephant bones (as Daubenton had already done in 1762) could have answered all of them so conclusively as to end the discussion. But the questions are simple now only because they have been answered. Every answer was contrary to the accumulated lore of all the millenniums before 1700. They required not only the rejection of some of the fondest beliefs of mankind but also the development of fundamentally new ways of thinking and of an apparatus for scientific interpretation. It was the great achievement of the eighteenth century that it made this revolutionary advance, even more basic than that wrought by the doctrine of evolution in the nineteenth century, and so made a science of paleontology possible. The concrete results are meager compared, say, with those so quickly obtained by Cuvier after 1800, but the expansion of the edifice to include so much greater a range of materials was possible only after the less showy foundation had been firmly constructed.

FIG. 9. The first figure of an American vertebrate fossil, from Guettard, 1756 (drawn by J. Ingram). Not identified by Guettard, the tooth is now identifiable as a third molar of the American mastodon. (The smaller figure shows a piece of rock from near Quebec with Paleozoic brachiopods, D, identified by Guettard as fossil moths.)

Guettard (1756, read in 1752) published the first illustration of an American vertebrate fossil in a curious memoir intended to demonstrate that the geological constitution and products of Switzerland and of North America are essentially parallel. He included in this memoir what deserves, despite its crudity, to be called the first geological map of the New World (Fig. 8). He gave two artistically good and relatively accurate views of a third molar of *Mammut americanum* (Fig. 9), certainly from the Longueuil collection although no statement of its origin was given beyond saying that it was "d'un endroit qui est marqué dans les cartes du Canada, sous le nom de canton où l'on a trouvé des os d'éléphant." The place is not so marked on Guettard's own map of Canada (Fig. 8), but the reference is clearly to Bellin's map (Fig. 4) or others copied from it, on which Longueuil's discovery is indicated. As for identification, Guettard asked, "De quel animal est-elle? & ressemble-t-elle aux dents fossiles de cette grosseur, qu'on a trouvées dans différens endroits de l'Europe?" His only answer was, "Ce sont-la deux points qu'il ne m'a pas été possible d'éclaircir."

A decade later Daubenton (1764, read in 1762) felt that he could clear up these questions. Dealing primarily with the femur taken to France by Longueuil, Daubenton attempted to elucidate its relationships by a figure in which this femur, one from a Siberian mammoth, and one from a recent elephant, are compared (Fig. 10). This is an excellent example of the comparative method for identifying fossils, a procedure that seems quite obvious now, but that was long in being adopted. No longer was it possible to call mastodon bones those of human giants: with simple genius Daubenton observed that the bones of different animals have characteristic forms, more or less constant for any one kind of animal, and that one can de-

termine whether two homologous bones are from the same species by setting them alongside each other and looking.

This is one of the four most basic discoveries or principles in the rise of vertebrate paleontology

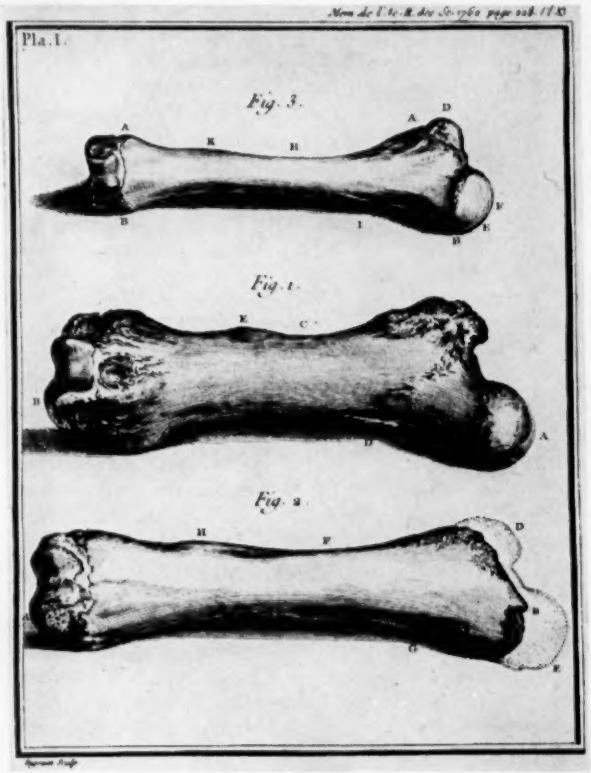

FIG. 10. Comparison of femora of the American mastodon (Fig. 1, middle), Siberian mammoth (Fig. 2, below), and a recent elephant (Fig. 3, above), from Daubenton, 1764 (drawn by Ingram). Although used by Daubenton to support the incorrect thesis that all three femora represent the same species, the plate is a unique early example of correct procedure in identifying fossil bones.

and it may fairly be dated from Daubenton, although even he had less important predecessors (such as Catesby's slaves) and although it is usually credited to Cuvier a generation later.[14] The others of these basic points are that fossils may and usually do represent extinct species, that they occur in definite, temporal sequence, and that they record evolutionary phylogenies. Some of the arguments regarding extinction will be considered on later pages. The concept of sequence was developed by stratigraphers and invertebrate paleontologists and gradually taken over by vertebrate paleontologists around the beginning of the nineteenth century. The evolutionary concept was not an accepted or leading principle in vertebrate paleontology until well after the period covered by the present paper.

The occurrence of extinct species was not envisioned by Daubenton or accepted by his authoritative contemporaries. From his point of view, therefore, the problem was to find the living animal whose bones might include some like those from the Ohio as variants. In this he was completely successful, because he demonstrated that the Ohio River (or Canadian, as he considered it) femur, the Siberian mammoth femur, and the recent elephant femur are all variations of the same type. To criticize the error of supposing that all represent a single species would be to insist that Daubenton should have used premises that were not developed, were scarcely even imagined, when he wrote.

The tusks confirmed Daubenton's conclusion, but the molar teeth (which were what we now call mastodon, and not mammoth, teeth) plunged him into a dilemma. They certainly could not be simple variants of the elephant patteern but must represent a distinct species. He concluded that the molars were those of a large hippopotamus. Even this grossly erroneous conclusion cannot be called absurd. Mastodon molars are, indeed, more like those of a hippopotamus than of an elephant, but here the comparative method necessarily failed as long as the premise of non-extinction was accepted. To bolster his conclusion that the molars, on one hand, and the tusk and femur, on the other, belonged to different animals, Daubenton insisted at some length that the bones were found by savages, incapable of judging or guaranteeing their association. He was right in rejecting the *necessity* of association as the materials reached him, but entirely wrong in rejecting the *possibility*. Here the limits of his scientific imagination, or of that of any of his learned contemporaries, had been reached. Buffon, the pope of eighteenth century zoologists, adopted and publicized Daubenton's conclusions, both right and wrong.

[14] He may justly be honored as the father of vertebrate paleontology, but it is improbable that Cuvier originated any of the truly fundamental principles of that science. He developed certain of these principles, exemplified them, systematized them, and for the first time brought together a considerable mass of material data. These great achievements warrant and assure his pre-eminent place in scientific history without claiming for him priority on particular points that were definitely anticipated.

These views did not, however, long hold the field undisputed. In 1767, as has been related, collections of mastodon bones and teeth became available in London and the English naturalists then began to study them. Franklin's views of that year are seen in his letter to Croghan, copied on a previous page. They exhibited his usual sound common sense and reasoning powers. He never doubted that all the remains were from one kind of animal and that this animal was a sort of elephant, but a sort with different grinders and probably carnivorous in habit. Elephants are now tropical. The occurrence in a wintry region of remains of similar animals must indicate either that the climate has changed radically or that the remains belong to an allied, extinct species of different climatic habit. The second alternative was closed for Franklin because, like Daubenton, Buffon, and many others, he had no good reason to doubt the accepted doctrine that animals do not become extinct. He therefore concluded that the earth had anciently been in another position and its climates differently placed. This was daring but truly scientific imagination, such as Daubenton lacked. In this conjecture Franklin also remarkably anticipated a modern geological theory.

Early in the following year Franklin had clarified and modified his views on the affinities and habits of these animals, as the following letter to Abbé Chappe shows[15] (Copied from Franklin's draft, Fig. 11, in the Library of the American Philososophical Society, Franklin Papers, Vol. 45, No. 33b; it has frequently been printed in Franklin's works).

Abbe Chappe

London Jan. 31. 1768

Sir

I sent you sometime since, directed to the Care of M. Molini, a Bookseller near the Quay des Augustins a Tooth that I mention'd to you when I had the Pleasure of meeting with you at the Marquis de Courtanvaux's. It was found near the River Ohio in America, about 200 Leagues below Fort du Quesne, at what is called the Great Licking Place, where the Earth has a Saltish Taste that is agreeable to the Buffaloes & Deer, who come there at certain Seasons in great Numbers to lick the same. At this [Pla]ce have been found the Skeletons of near 30 [larg]e Animals suppos'd to be Elephants, several Tusks like those of Elephants being found with these Grinder Teeth. Four of these Grinders were sent me by the Gentleman[16] who brought them from the Ohio to New York, together with 4 Tusks, one of which is 6 Feet long & in the thickest Part near 6 Inches Diameter, and also one of the Vertebrae. My Lord Shelbourn receiv'd at the same time 3 or four [of] them with a Jaw Bone & one or two Grinders remaining in it[.] Some of Our Naturalists[17] here, however, contend, that these are not the Grinders of Elephants but of some carnivorous Animal unknown, because such Knobs or Prominances on the Face of the Tooth are not to be found on those of Elephants, and only, as they say, on those of carnivorous Animals. But it appears to me that Animals capable of carrying such large heavy Tusks, must themselves be large Creatures, too bulky to have the Activity necessary for pursuing and taking Prey, and therefore I am inclin'd to think those Knobs are only a small Variety.[18] Animals of the same kind and Name often differing more materially, and that those Knobs might be as useful to grind the small Branches of Trees, as to chaw Flesh. However I should be glad to have your Opinion, and to know from you whether any of the kind have been found in Siberia.

With great Esteem & Respect, I am—

Sir,

Your most obed[t] hum[l]

Servant

B. F.

In his correct reversal of opinion regarding the diet of the animal (considered carnivorous in the earlier letter to Croghan), Franklin was perhaps influenced by Collinson, with whom, instead of Hunter, he now agreed (see below). It is not surprising that Franklin's relative unfamiliarity

[15] Abbé Chappe d'Auteroche (1722–1769) was a noted French astronomer who went to Tobolsk to observe the transit of Venus in 1761. At the time of Franklin's letter he was being lionized as an authority on the little-known wastes of Siberia and his book on that region appeared later in the same year, 1768.

Franklin's letter incidentally clears up a mystery that plagued Cuvier and that has, as far as I know, been ignored and not explained by later authors. Buffon (1778, Pl. III) had figured a tooth of what we now call *Mammut americanum* with the statement that it was from Siberia. Cuvier (1834, p. 257) knew that this species was quite distinct from the Siberian mammoth and he believed that it was confined to North America, but he was baffled by this record of a Siberian specimen. The tooth was from the Chappe collection, and was therefore *assumed* to be from Siberia, where Chappe had traveled and had collected mammoth teeth. Cuvier suggested that Chappe might have obtained the specimen in California, which he had also visited. It seems to me beyond serious doubt that this was the very tooth that Franklin had sent to Chappe and that it was therefore collected by Croghan at Big Bone Lick.

[16] [Croghan.]

[17] [Referring probably to Hunter.]

[18] [That is, a minor variation on the elephant molar pattern.]

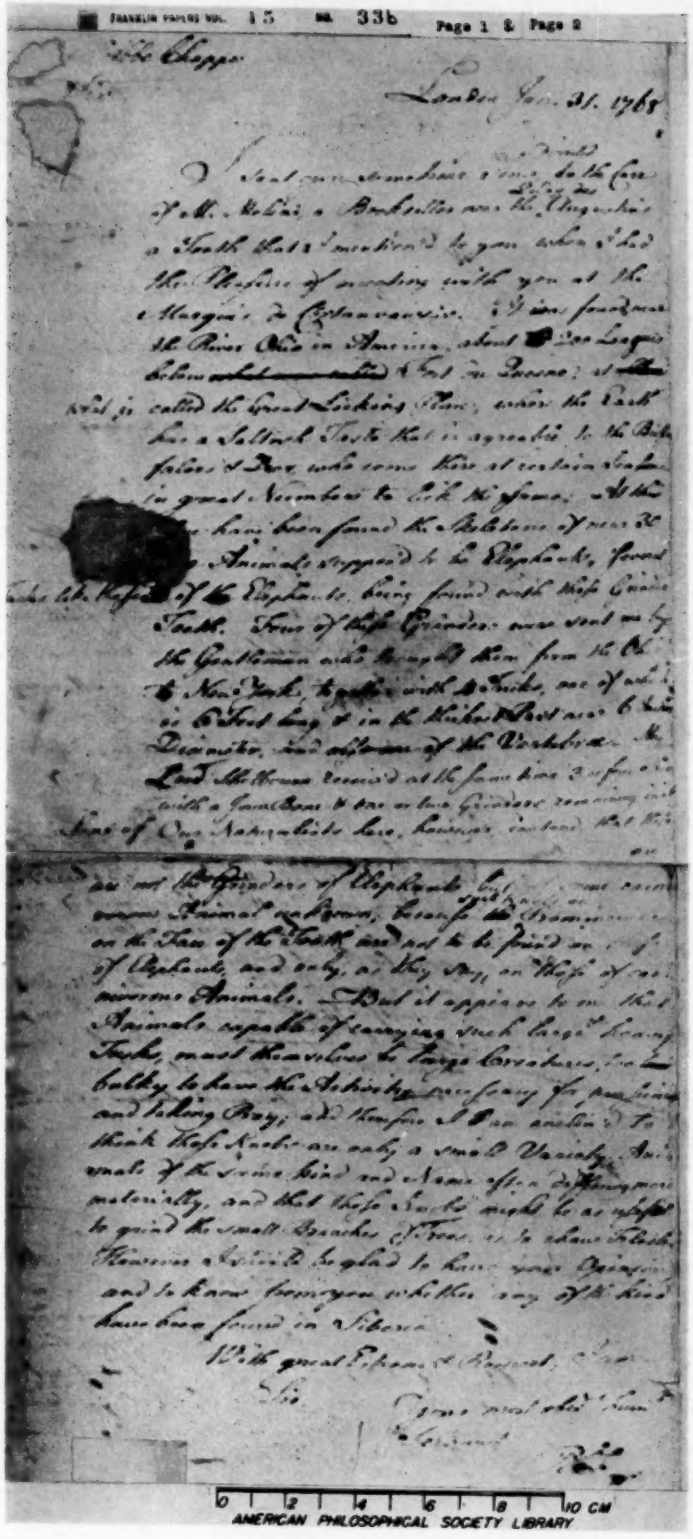

FIG. 11. Draft of letter from Dr. Benjamin Franklin to Abbé Chappe d'Auteroche, Jan. 31, 1768, discussing the mastodon remains sent to Franklin by Croghan. From the original, Library of the American Philosophical Society.

with the species concept of the day led him to a gross overestimate of the extent of variation allowable within a single species, according to contemporary as well as modern standards.

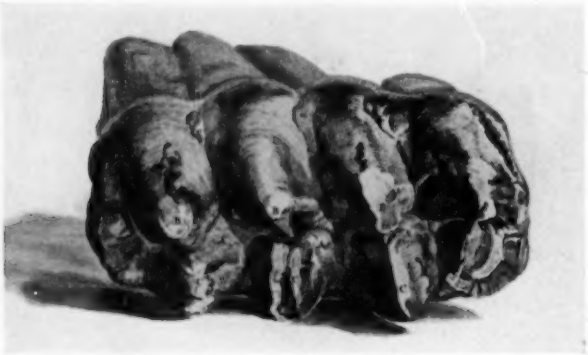

FIG. 12. Collinson's figure of an American mastodon molar, probably one of those collected by Croghan in 1766 at Big Bone Lick. From Collinson, 1768.

The first English technical memoir on these discoveries was read to the Royal Society by Collinson on Nov. 26, 1767, and published in the following year (Collinson, 1768a). He described the occurrence at Big Bone Lick in substantial agreement with the data in Croghan's journal and letters. The tusks, said Collinson, are those of elephants, but—

> It is very remarkable, and worthy observation, none of the molares, or grinding teeth of elephants, are discovered with these tusks; but great numbers of very large pronged teeth of some vast animals are only found with them, which have no resemblance to the molares, or grinding teeth, of any great animal yet known.

The Ohio climate is too severe for elephants. In Siberia, said Collinson, the elephant remains may have been swept northward by wind and wave at the time of the Deluge, but how anything can account for the remains that occur in America was submitted to the learned Society as an unsolved problem.

Having titillated his colleagues by posing these questions, Collinson gave answers to them at the next meeting, Dec. 10, 1767 (Collinson, 1768b). He had compared the tusks with those of African and Asiatic elephants and found that the fossils agreed in all respects with some of these—

> But as the biting or grinding teeth, found with the others, have no affinity with the molares of the elephant, I must conclude, that they, with the long teeth [tusks], belong to another species of elephant, not yet known; or else that they are the remains of some vast animal that hath the long teeth, or tusks, of the elephant, with large grinders peculiar to that species, being different in size and shape from any animal yet known. . . .
>
> The elephant is wholly supported by vegetables; and the animal to which these grinding teeth belong, by their make and form, seemed designed for the biting and breaking off the branches and trees for its sustenance. . . . This great creature, to which these teeth belong, wherever it exists, is probably supported by browsing on trees and shrubs, and other vegetable foods.

Collinson avoided the shocking idea that the animal might be extinct, but he seems to have been on the verge of this great innovation. He explicitly made three important and brilliant contributions: the tusks and the grinders belonged to the same animal; this animal was of a species perhaps elephantine but distinct and known only from fossils; by analogy of affinity and structure this animal was herbivorous. It was many years before these correct and well-reasoned conclusions were generally accepted.

At the same time as Franklin and Collinson, the equally gifted William Hunter was studying the Croghan collection and also another large collection in the Tower. He presented his conclusions to the Royal Society on Feb. 25, 1768 (Hunter, 1769). He made elaborate comparisons of the tusks and called in skilled ivory workers, who pronounced them "perfectly similar" to the tusks of recent elephants. "Yet their opinion, and what I saw with my own eyes, convinced me of this fact only, viz. that true or genuine ivory is the production of two different animals, and not of the elephant alone."

Hunter compared the lower jaw collected by Croghan[10] with one from an elephant, using Daubenton's method with more perception and precision (Fig. 13), and he proved with complete finality that the species are different. The mastodon (as we say now) was called a *"pseud-elephant," "animal incognitum,"* and "American *incognitum."* He imagined that the Siberian (*i.e.* northern mammoth) bones would prove to be of the same kind and gave some reasons for thinking so, but admitted that he did not really have the materials necessary for proper discussion of this question. If he had had the materials, Hunter's methods and ability would have avoided this error

[10] Which had lost one of its two molars between the time of its receipt by Lord Shelburne and its study by Hunter.

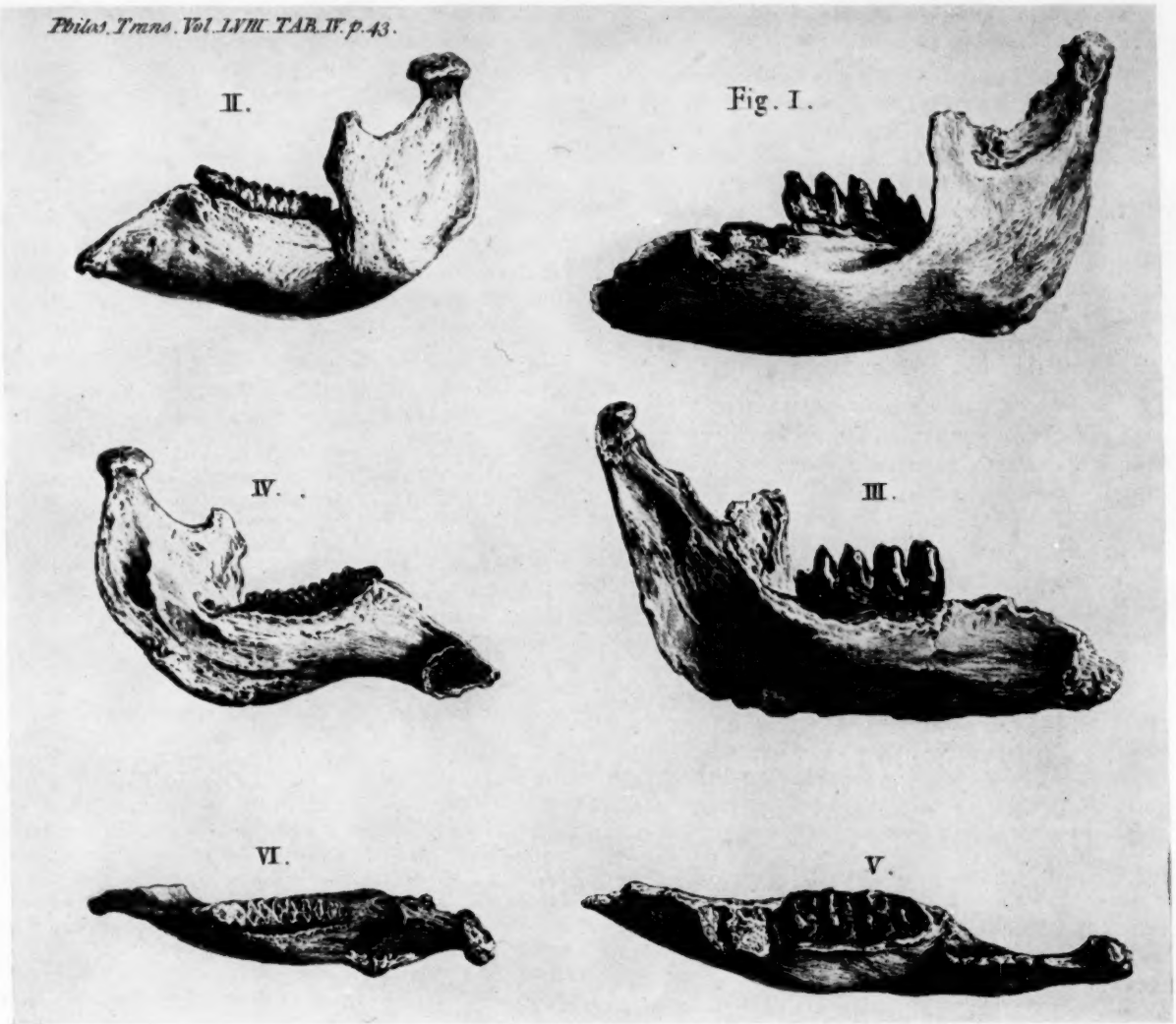

FIG. 13. Jaw of an American mastodon, I, III, V, compared with that of an African elephant, II, IV, VI, from Hunter, 1768 (drawn by Riemsdyk). The plate was introduced by Hunter to demonstrate that the "American *incognitum*" was not an elephant but a distinct species, probably extinct, a correct but, at the time, radical and generally rejected conclusion.

and cleared up this remaining ambiguity. As to the habits of the animal, Hunter went astray where Collinson had succeeded and Franklin had been persuaded: he unfortunately gave the weight of his authority to the carnivorous theory.

As the *incognitum* of America has been proved to have been an animal different from the Elephant, and probably the same as the Mammouth of Siberia; and as grinder teeth like those of America have been dug up in various other parts of the world, it would seem to follow, that the *incognitum* in former times has been a very general inhabitant of the globe. And if this animal was indeed carnivorous, which I believe cannot be doubted, though we may as philosophers regret it, as men we cannot but thank Heaven that its whole generation is probably extinct.

Thus quietly, almost inadvertently, Hunter closed his discussion with a conclusion that was to startle the world when Cuvier again reached it thirty years later: fossil bones represent animals probably extinct. The novelty and impact of this idea, so hard to comprehend now that it has become a commonplace, are emphasized by Jefferson's eloquent expression of the orthodox view

more than a decade later (1781–82, quoted from Jefferson, 1825, p. 73):

> It may be asked, why I insert the mammoth [mastodon] [in a list of animals common to Europe and America], as if it still existed? I ask in return, why I should omit it, as if it did not exist? Such is the economy of nature, that no instance can be produced, of her having permitted any one race of her animals to become extinct; of her having formed any link in her great work so weak as to be broken. To add to this, the traditionary testimony of the Indians, that this animal still exists in the northern and western parts of America, would be adding the light of a taper to that of the meridian sun.

The rest of Jefferson's discussion at that time also did little to promote the subject beyond indicating that mastodon remains were becoming fairly common and the localities multiplying. A locality on the North Holston, a branch of the Tennessee, was explicitly mentioned and others more vaguely. Big Bone Lick dominated American vertebrate paleontology for another fifty years, but it hereafter ceased to be the only important fossil locality in the records.

Jefferson controverted Daubenton's belief that the mastodon remains are from two different animals or that they belonged either to the elephant or to the rhinoceros—points already sufficiently settled by Collinson and Hunter—and he espoused Hunter's conclusion that the animal was a carnivore.

It is curious that regarding one aspect of the problem Jefferson reached at this time the correct solution, and was apparently the first to do so, but that he reached it on erroneous grounds. He explained the northern distribution of mammoths and mastodons by the affirmation that they were not elephants, which are tropical, but a very different species adapted and peculiar to cold climates.

Indeed after Hunter the subject stagnated both in Europe and in America, with some repetition and discussion of what had already been done, but with no important original contributions in the published records until well into the 1790's. In 1792 another landmark was passed when Kerr applied a technical Linnaean name to the animal, *Elephas americanus*, without otherwise contributing to knowledge of it. This specific name, now recognized as prior and valid, was almost completely overlooked during the late eighteenth and early nineteenth centuries, when there was a spate of synonyms.[20]

A next step in unraveling this mystery, and almost the first taken by an American on his own ground[21] was by George Turner in a paper read to the American Philosophical Society July 21, 1797, and published by it in 1799.

He correctly recognized the existence of two kinds of "incognita"—a striking advance over current opinion. He concluded that the name mammoth properly applied to the animal that we now call a mastodon, that it was "carnivorous or mixed" in diet, and that it had claws. "With the agility and ferocity of the tiger; with a body of unequalled magnitude and strength, it is possible that the Mammoth may have been at once the terror of the forest and of man!—And may not the

---

[20] Cuvier called this and allied forms "mastodontes" in the vernacular and the name survives both colloquially and in the technical form *Mastodon*. The prior generic name is, however, *Mammut* Blumenbach, 1799. Cuvier's "mastodonte" was not proposed until 1806 and *Mastodon* was not used as part of a Linnaean binomial until 1814 (Rafinesque). *Mammut* perpetuates the old confusion of the American mastodon and the Siberian mammoth and is inconvenient because of its apparently erroneous connotation. Many students therefore refuse to use it, without denying its technical validity. Nevertheless the generic name *Mammut* for the American mastodon has some justification even beyond its unquestioned priority. Rembrandt Peale (1803b) was well aware that the two animals are not the same, but he argued at some length that the American, not the Siberian, animal should be called "mammoth." "Mammoth," he maintained, merely meant a large, legendary animal. Tales of the pretended mammoth of Siberia *"as an animal so called,* are entirely fabulous." The real fossil animal found in Siberia, in fact a sort of elephant, was called "mammoth" without more right than the real fossil animal of America, also called a "mammoth" for 60 years by thousands ignorant of the origin of the word. The American animal is not an elephant and had no name [when Peale wrote] unless it continued to be called a mammoth.

Peale's argument from usage is, of course, now reversed and Cuvier's (later) name mastodon has long had the sanction of custom, but consideration of Peale's statements and of its precedents does show that *Mammut,* for the American mastodon, has a historical background forgotten by most of its present opponents and perhaps in some measure compensating for its admitted inconvenience.

In this study I use the legally correct and historic name *Mammut americanum.*

[21] Excepting Franklin as having been in London and in contact with English naturalists there, and Jefferson as having made no real contribution to the solution. The Reverend Robert Annan wrote what might be called a paleontological memoir in 1785 and this was published in America in 1793, but he did not advance knowledge in any important way. He shrewdly guessed that his fossils, found on the Walkill River (north of New York City) in 1780, might be similar to some supposed elephant bones found in England, but he was unaware of the solution to this problem achieved by the English savants more than fifteen years earlier.

human race have made the extirpation of the terrific disturber a common cause?"

The second type of "incognitum," now called the mammoth but unnamed by Turner, was correctly inferred to be herbivorous. Two kinds of tusks were recognized, really corresponding with mammoth and mastodon tusks, but it was denied that either kind could belong to the carnivorous "mammoth" (*i.e.*, mastodon). Turner opposed the authoritative view of Jefferson and others who denied the existence of extinct animals:

"We are now acquainted with the skeletons of five several large animals,[22] all of which are, at present, unknown [as living animals]: and as two of those skeletons were but recently brought to light, may we not expect to be gratified, in these times of research, with other discoveries of a similar kind? Can we believe, then, that so many and such stupendous creatures could exist for centuries and be concealed from the prying eye of inquisitive man?" Turner was a better prophet and philosopher than paleontologist.

So far from the vanished milieu, it is difficult for the historian to know how much was a subject of conversation among the learned, perhaps even common knowledge, before it reached cold print and appears in retrospect as a discovery. Thus it is impossible to be sure how much credit attaches to Turner's recognition of the two species previously confused in the literature and to his insistence that both were extinct, especially when these points must be singled out, in the light of present knowledge, from the midst of so much that verged on the nonsensical when it was written and now seems utterly absurd. Whatever the personalities involved, the memoir does prove that the final solution to the problem was at least in the air, so to speak, in American scientific circles.[23]

Whatever its philosophical merits, Turner's paper cannot be called scientific and it was Cuvier who at about the same time (1799 and subsequently) produced all the essentials of the final solution: The elephant-like animals then known from North America were really of two kinds. Both were related to the living elephant but both belonged to separate, wholly extinct species. One, the "mammoth" of the Americans, the "mastodonte" of Cuvier (in 1806 and later), was quite unlike the mammoth of Siberia. The other was like, if not identical with, the latter. The mammoth of Siberia was also extinct and not of the same species as any living elephants. All these forms were herbivorous. They could live in the north because they were northern species and not the same as the tropical forms of today. It was Cuvier's good fortune that he had the materials and it was the world's good fortune that he had the intelligence to discriminate the true and the false in all the preceding work of the eighteenth century and to unify and amend these facts and inferences into a statement of the case that has proved permanently valid, aside from unimportant details.

[22] The two "incognita," now called mammoth and mastodon, the large bones from a cavern in Virginia, now called *Megalonyx*, and the South American megathere. For the fifth, Turner was probably counting the megathere twice (once as such and once as what he curiously calls "The Megolicks of Paraguay"), or referring to some allied form (G. G. S., note).

[23] George Turner, "one of the Judges of the Western Territory," was elected a member of the American Philosophical Society Jan. 15, 1790. In or about 1792 he apparently moved to Philadelphia and was thereafter one of the most constant and active participants in meetings of the Society until the beginning of 1800 when in rapid succession he was elected a curator, defrauded the Society of $500, and was expelled from it.

Wistar and Jefferson

"But it is ever to be regretted, that the principal cultivators of natural science, in the United States, are professional characters, who cannot, without essentially injuring their best interests, devote to these subjects, that sedulous attention which they demand. . . . In *some* respects, they are, certainly, better qualified to undertake, and to perform, the task than the naturalists of Europe."—Benjamin Smith Barton, 1804.

Until near the turn of the century, American vertebrate paleontology was almost solely concerned with the mammoth and mastodon and almost all the important studies were made in Europe, by the French or English. Late in the 1790's began the expansion that was to continue so enormously to our own day. American fossils began to be found in greater variety and American naturalists began to study them.

In the long roster of American vertebrate paleontologists, the name of Caspar Wistar should stand first in time. He published only two short papers in this field, but one of these was the first technical study of professional quality to be written by an American or to be published in America. He also displayed an interest and exerted an influence disproportionate to the paucity of his published work. It is ironic that the traditions of later vertebrate paleontology should have trans-

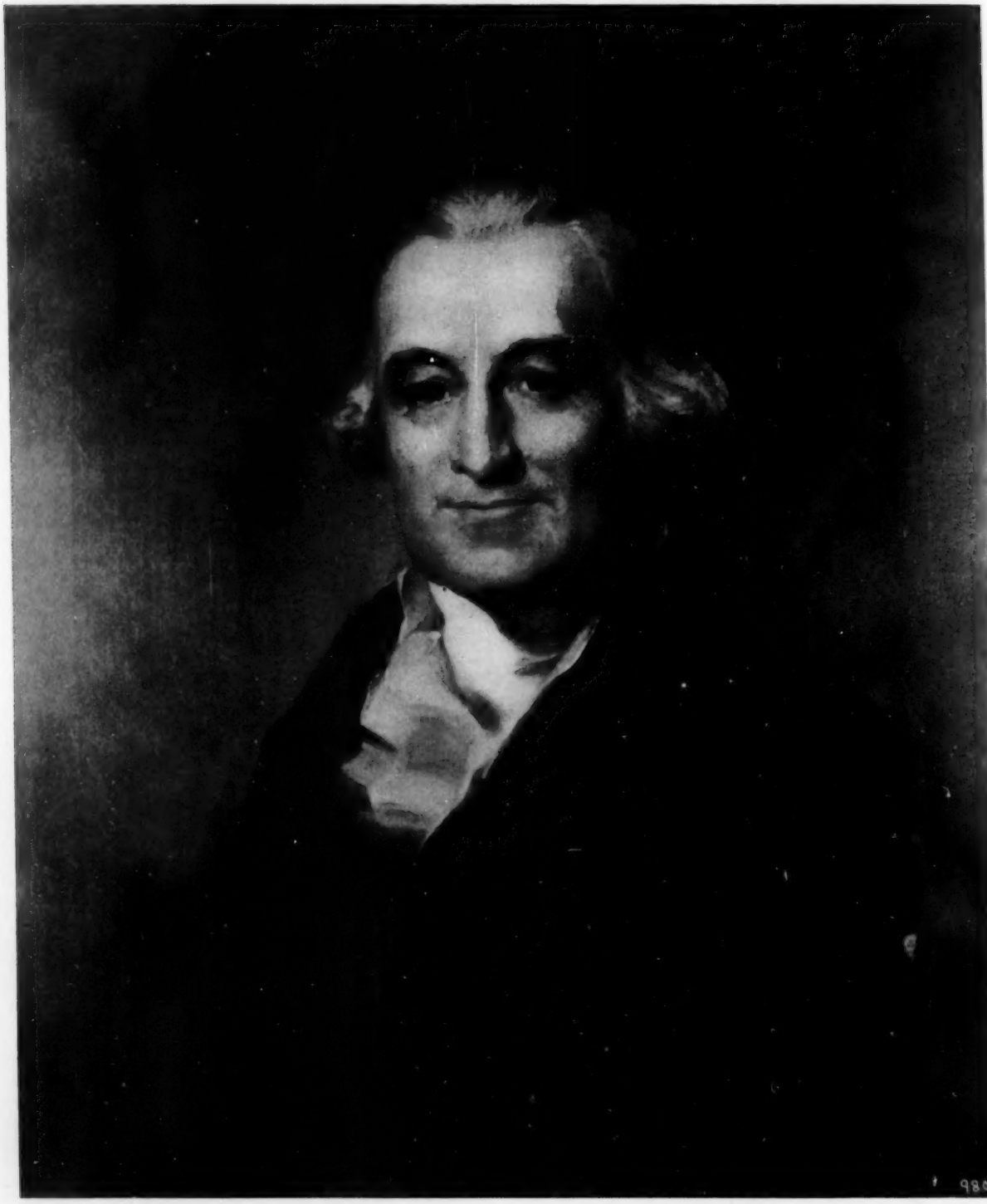

FIG. 14. Dr. Caspar Wistar, President of the American Philosophical Society, first American author of competent, technical studies of fossil bones. Portrait by Sully, from the American Philosophical Society.

ferred to Jefferson much of the credit that Jefferson, himself, rightly granted to Wistar.

Wistar was born in Philadelphia on Sept. 13, 1761, and like most of the scientists of his and the immediately following period he received a medical education,[24] in Philadelphia, London and Edinburgh. Returning to Philadelphia, he taught in the forerunners of the University of Pennsylvania and was Professor of [human] Anatomy there after 1808. In 1811 he published the first American textbook of anatomy, but his was not an age of voluminous publication or of evaluation by bibliography and his printed studies in other fields were almost as scanty as in paleontology. When scarcely twenty-six years old, in 1787, he was elected a member of the American Philosophical Society, which thereafter was a dominating influence in his professional and social life and on which he, in turn, had a strong and beneficial effect. He became a curator of the Society in 1793, vice-president in 1795, and was president from 1815 until his death on Jan. 22, 1818. It is significant that of the four great early presidents of the Society, Franklin, Rittenhouse, Jefferson, and Wistar, all but Rittenhouse were at some time particularly concerned with vertebrate paleontology.[25]

Almost immediately after his election to the Society, on Oct. 5, 1787, Wistar, jointly with Timothy Matlack,[26] presented a paper on a large thigh bone found near Woodbury Creek in Gloucester County, New Jersey (*Early Proceedings*, p. 154). Matlack, Wistar, and Rogers were directed to search for the rest of the skeleton, but neither the results of the search nor their opinions on the bone are recorded. This may have been the first dinosaur bone to come to learned attention. Thereafter Wistar became the Society's usual authority on fossil bones. He was, for instance, referee for Turner's paper of 1797, discussed on a previous page, and after Jefferson's treatment of them, the *Megalonyx* bones were turned over to Wistar for further study.

This was the occasion for Wistar's first formal paleontological memoir, completed and published in 1799.[27] This paper is a model of cautious, accurate scientific description and inference, an achievement almost incredible in view of the paleontological naïveté of his associates and of the lack of comparative materials. The objective part of the paper is so clear, complete, and correct that it has never been significantly bettered for the same or similar objects.

The available specimens included a radius, an ulna, three claws, and half a dozen more proximal toe bones (Fig. 15). They had been found, as Jefferson (1799) recorded, in Greenbriar County, Virginia [now in West Virginia], by workmen excavating for niter (or saltpeter) in the floor of a cave belonging to one Frederic Cromer. Some of the bones were dispersed as curiosities, but some were saved by Colonel John Stewart, of Greenbiar County, and a Mr. Hopkins of New York. These salvaged remains were brought together by Jefferson and presented to the American Philosophical Society. What Jefferson, himself, made of them will be mentioned later. It is typical of the roles of Wistar and Jefferson in this history that the latter obtained the specimens and that the former interpreted them.

Although imbued with scientific caution to a degree altogether exceptional in that day and far from universal in ours, Wistar did not stop at simple description but drew physiological and taxonomic inferences. From the radius and ulna, he concluded that the palm of the hand would present inwards, not downwards or backwards. From the shortness of the metapodials and the form and arrangement of the other known foot bones, he inferred that the animal did not walk on its toes and that the last phalanx was not re-

[24] Of the eleven Americans who (in my opinion) made the most significant contributions to the literature of paleontology in 1799–1842, nine were educated as physicians, one had been a saddler by profession (W. C. Redfield; S. W. Peale, not counted among these eleven but otherwise important in paleontological history, was also trained as a saddler), and one was trained for the ministry (Edward Hitchcock).

[25] Wistar's successor at the University of Pennsylvania, Horner, also shared this interest. In passing it may be noted that Wistar's anatomical collections were the mucleus of the Wistar and Horner Museum of the University of Pennsylvania, merged with the Wistar Institute in 1892, and that contrary to what seems to be a common belief the Wistar Institute was not founded by Caspar Wistar but by his great-nephew Isaac Jones Wistar. Caspar Wistar's life is well summarized by Packard, 1936, who gives references to other biographical studies.

[26] Matlack is one of the forgotten worthies of vertebrate paleontology, with which he was periodically concerned on behalf of the American Philosophical Society. He was one of the earlier members and was Secretary of the Society in 1781–1783. He died in 1829. As far as I can learn, none of his paleontological studies has ever been published.

[27] Scott (1927) and others have understandably assumed that Wistar's memoir was read in 1797, like the others published in the same (1799) volume of the *Transactions*. The *Proceedings*, however, show that the paper was not submitted until March 1, 1799. It probably was printed without being read to a meeting.

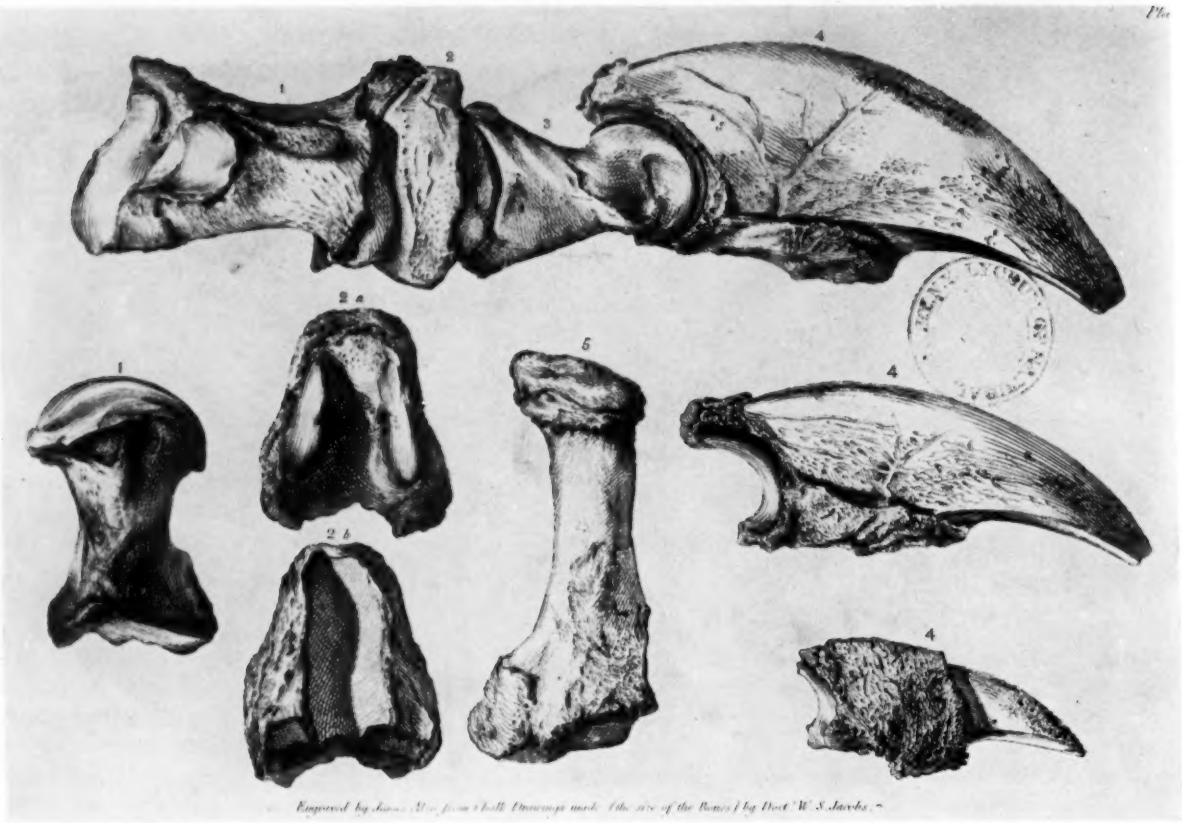

FIG. 15. Toe bones of *Megalonyx jeffersoni*, from Wistar's classic memoir, 1799. Drawn in chalk by Dr. W. S. Jacobs and engraved by James Akin for the *Transactions of the American Philosophical Society*. This illustration and the accompanying memoir marked the beginning of technical vertebrate paleontology in America. (Reproduced from the N. Y. Lyceum copy, now in the American Museum, used by L. S. Mitchill in his pioneer studies.)

tracted (as in the felines). This correct reasoning from such fragmentary remains of an animal totally new to human knowledge was unqualifiedly brilliant.

As regards the zoölogical affinities of the new animal, Wistar was still more cautious but no less successful. He observed that among living animals, "the megalonix," as he called it (after Jefferson), was most nearly comparable to the sloth, yet quite distinct. Available knowledge of the only previously discovered extinct giant sloth, *Megatherium*, was limited to one poor illustration, but Wistar observed that the animal from Virginia was not the same as the *Megatherium*. Since 1799 we have learned much more about *Megalonyx* than Wistar knew, but we have not had to correct any essential point in his account.

Accident and his habit of making verbal communications rather than printed publications have deprived us of some observations by Wistar that must have been equally brilliant and revolutionary. For instance on Jan. 17, 1806, he reported to the Society that a skull recently sent by Dr. Sam Brown from a cave in Kentucky belonged to a peccary (*Early Proceedings*). This first identification of a fossil peccary was correct (as shown by Leidy's later description and figure of the same specimen) and it long antedated the generally accepted time of this discovery.

During 1806 and 1807 Wistar collaborated in Jefferson's efforts to obtain collections of bones of the mastodon and associated animals and when these efforts were so richly successful (as will be related in due course), Jefferson sent for Wistar (letter of March 20, 1808), who went to the White House and studied the collection. Wistar's observations on the Jefferson-Clark collection were communicated to the Philosophical Society on Feb. 3, 1809. This paper was to have been published in the *Transactions*, but it was lost by the com-

mittee of referees and an advertisement failed to recover it. It was in part rewritten and what was intended as the beginning of the description of the Jefferson-Clark collection appeared in the *Transactions* in 1818.[28] Wistar died before the paper was printed and his other observations on the collection died with him.

This 1818 paper is briefer and of less fundamental importance than Wistar's memoir on *Megalonyx,* but it also exemplifies his skill. Describing two incomplete skulls, he demonstrated that the first was a large deer of some sort but distinct from the most nearly comparable living forms, the wapiti and the moose. Harlan subsequently gave the fossil a specific name, *Cervus americanus.* The species is valid but the name was a homonym; the animal is now called *Cervalces scotti.* The generic name *Cervalces* Scott reflects the mingling of characters of *Cervus* and *Alces* observed by Wistar. The second skull was referred to the genus *Bos,* which then included all the bovines, and was shown to be sharply distinct but believed to be allied to the bison. Harlan later named the species *Bos bombifrons* and Leidy removed it to a new genus, *Boötherium.* It is now known to be more nearly allied to the muskox, a form of which comparative material was probably not available in Philadelphia. Although the identification by Wistar was less brilliant than his other work, the error, if it can be called such, was one of degree and not of kind.

An important feature of Wistar's work is that he was not dealing with the mastodon, already made relatively familiar by some sixty years of study, but with extinct species that were entirely unknown to science before they came into his hands and that represented distinctly more difficult problems of identification and relationships. Jefferson had brought together these specimens, had presented them to the Society, and in this sense was responsible for the increased diversity of known American fossil mammals.

Thomas Jefferson, whose life is too well known to need any summary here except in its paleontological aspects, has become a fabulous figure to paleontologists, few of whom know what he really did but most of whom consider him as the father or founder of vertebrate paleontology in America, as the discoverer and nomenclator of *Megalonyx,* and so on. Jefferson was surely one of the greatest men that his race and nation have ever produced. It should not be considered iconoclastic (although I have already learned that it seems so to my colleagues) to state that he was not a vertebrate paleontologist in any reasonable sense of the words, that he never collected a fossil or gave one a technical name, and that his scientific contributions were negligible or retrogressive. Jefferson, himself, was perfectly aware that he had no accomplishments as a student of paleontology, or as a research scientist, and in saying so one is not rejecting any pretensions of his, but only attacking a subsequent false legend.[29]

Jefferson was, nevertheless, an important figure in the rise of vertebrate paleontology, to which he made two contributions such as no other man of the time could or did make: he helped to make paleontology a respectable and honored pursuit, and he was largely instrumental in bringing together the materials necessary for its advancement.

As the foremost citizen of the young nation, Jefferson's outspoken and excited interest in fossils conferred on their study the dignity and prestige inseparable from his personality and position. The materially shrewd and self-styled "practical" American tradesmen, artisans, and farmers tended to look down on the "useless" pursuit of pure science, as some of them still do. His worst enemies could not consider Jefferson a fool, and it had to be granted that a subject could not be wholly worthless when he proclaimed it valuable. For years he lost no opportunity to obtain and examine bones, and for a time a room in the White House was especially set aside and piled high with them.

This matter of prestige did, indeed, work both ways. If Jefferson uplifted the status of paleontology, he did not escape condemnation for his association with this distrusted subject. Thence the once suppressed but often quoted stanza by William Cullen Bryant:

> Go, wretch, resign thy presidential chair,
> Disclose thy secret measures, foul or fair,

[28] This rewritten version apparently was not read to the Society. It must have been completed in 1817.

[29] The necessary correction of this legend extends also to contradiction of the usual belief that Jefferson was the only or the first president of the United States to be concerned with vertebrate paleontology. Priority in this respect goes to the limit of possibility; it belongs to no less a figure than George Washington. Speaking of what we now know to have been mastodon remains, collected in 1780, Robert Annan wrote (1793, p. 164): "His Excellency, General Washington, came to my house to see these relicts. He told me, he had in his house a grinder which was found on the Ohio, much resembling these."

Go search with curious eyes for horned frogs,
'Mid the wild wastes of Louisianian bogs;
Or Where the Ohio rolls his turbid stream
Dig for huge bones, thy glory and thy theme.

The precocious poet was then a child of thirteen and this ridicule of his betters was doubtless inspired by his elders, who were at this time, and particularly in Bryant's environment, much exercised by Jefferson's embargo policy (an early precedent for appeasement, so opposed by Jefferson's mooted political heirs).

In his role as patron of science, Jefferson greatly encouraged the study of vertebrate paleontology by the American Philosophical Society while he was its president (1797–1814). He also acted for a time as president of the board of trustees of Peale's Philadelphia Museum, which, as related elsewhere, included the first public exhibition of fossil vertebrates and the first mounted fossil skeleton in America.

Jefferson's' other, equally important rôle in the history of paleontology was well expressed by himself as early as 1782 in a letter to Mr. Steptoe: "A specimen of each of the several species of bones now to be found, is to me the most desirable object in natural history."[30] Jefferson's efforts towards this end were constant and fruitful from about 1796 to 1807. His other duties, including the presidency of the United States, inevitably prevented any personal participation in the search and there is no indication that he even saw a bone *in situ* or collected one, but he was instrumental in saving many specimens that would otherwise have been dispersed and he actively promoted the search for them through his voluminous correspondence and through expeditions, at least one of which he personally financed. The bones thus gathered were invariably submitted to Wistar for study and identification and the majority of them were presented to the American Philosophical Society. The famous *Megalonyx* bones, already mentioned, were obtained by Jefferson through his correspondents, and, as will later be related, the Lewis and Clark expedition, sent out by him, acquired a few important fossils.

Jefferson's most important fossil collection, however, was that made for him, and at his expense, by Clark after his return from the far West. Jefferson had long been interested in Big Bone Lick and desirous of enlarging the Society's collection of mastodon bones, which by 1806 already included all parts except the skull. In 1804 Dr. William Goforth had made a large collection at Big Bone Lick and he had later conveyed this to Pittsburgh, with a view toward sale to the Philosophical Society or to Peale for his museum. Jefferson became interested in this collection, and in 1806 he and Wistar corresponded with Dr. Goforth concerning it, but in the meantime an adventurer variously known as Ash, Ashe, and Arvil had obtained them on consignment for sale. This individual conveyed the bones to England where he made a great stir with them and eventually sold them, but Dr. Goforth saw nothing more either of his bones or of the money that they brought. (The correspondence and other details are well given in Jillson, 1936.)

Although he no longer had any bones to sell, Dr. Goforth was greatly interested in obtaining more for Jefferson and the Society, and he proposed an expedition for that purpose. "I think," he wrote to Jefferson, "with 10 or 12 hands (who must be found, victuals, and liquor), I could completely search the whole Lick. The expense would be about $1.25 each man per day; we could take provisions from this town, or take a hunter to kill for us." (Although liquor is no longer accounted a legitimate part of collecting expenses, expeditionary costs have increased considerably.)

Jefferson refused this offer and instead made an arrangement with his acquaintance Mr. Ross, then owner of Big Bone Lick, and sent Captain Clark to do the collecting. During the summer of 1807 Clark worked with ten laborers for several weeks and forwarded the results to Washington. These bones, about three hundred in number, were the ones installed in the White House in 1808. Because the large bones of the mastodon were already well known, Clark concentrated attention on smaller bones and obtained an excellent representation of extinct ruminants. There were, of course, some mastodon remains as well as some of the mammoth (in modern terminology), but there were also fossil bison, muskoxen, and deer. This is the collection that Wistar examined in Washington in 1808 at Jefferson's invitation and later in Philadelphia, as already related. All specimens selected by Wistar were presented to the Society, and many of them are still in the Academy of Sciences of Philadelphia. Of the remainder, Jefferson retained a few and sent the rest to Paris, where some of them are now on exhibition in the

[30] Collected writings; also in Osborn, 1935, where most of the pertinent passages in Jefferson's letters are conveniently compiled, together with too enthusiastic a statement of Jefferson's paleontological accomplishments.

Muséum d'Histoire Naturelle in the Jardin des Plantes, or were immediately before the present war. After this major success Jefferson, who was 65 and considered himself old, seems to have taken little active part in paleontology. He returned to Virginia, became much concerned with the university, and had less opportunity for contacts with the Philosophical Society, retiring from its presidency and being succeeded by Wistar at the beginning of 1815.

Jefferson made only one attempt at personal identification of a fossil and wrote only one paper in this field, the famous memoir on *Megalonyx*, read in 1797 and published in 1799. Like most of Jefferson's writings, this paper is a model of eloquence and a pleasure as a literary production. As scientific research, it departs from inaccurate observations and proceeds by faulty methods to an erroneous conclusion. Jefferson decided that the bones were those of a great cat, some three times the size of a lion. The fact that the conclusion was wrong obviously does not, in itself, warrant characterizing the paper as unscientific, but the methods and viewpoint do.[31] After finishing the paper, and quite possibly on consulting with Wistar although this is not recorded, Jefferson saw a figure of *Megatherium* and recognized the possibility of relationships or identity between that animal and the megalonyx, but he unfortunately read his memoir as written and permitted it to be published in the original form two years later. It has already been shown that Wistar was successful where Jefferson had failed.

As a theorist or natural philosopher, Jefferson's slight paleontological work must also be regretfully dismissed as poor. Although he was one of the most enthusiastic amateurs of the bones of extinct animals, he never admitted that they were extinct. "For if one link in nature's chain might be lost, another and another might be lost, till this whole system of things should evanish by piecemeal" (Jefferson, 1799, pp. 255–256). Thus we unexpectedly find Jefferson, who did so much for paleontology in other respects, resolutely opposing the first great generalization on which paleontology depends as a science, a generalization already well known and accepted among his more strictly scientific colleagues.

[31] It may also be pointed out as a matter of record that the technical name *Megalonyx* Jefferson, so often cited, has no existence. Jefferson used the word "megalonyx" only as a vernacular equivalent of "great-claw," and not as the Linnaean name of a genus.

It is a measure of his greatness that Jefferson continued his powerful aid to paleontology and his warm friendship with its students even when it became evident that this aid and these students were revealing the falsity of views that he had vehemently and almost religiously expressed and maintained during the greater part of a long life.

Museums and the Peale Family

"The bones of the Mammoth first produced the idea of a Museum, which, after eighteen years of rapid approach to maturity, under the unprecedented exertions of an individual, has in its turn enabled you to place among its treasures nearly a perfect skeleton of the Mammoth—the first of American animals in the first of American Museums."—Rembrandt Peale, 1803.

Vertebrate paleontology is now primarily a museum subject. Indeed a collection of fossil bones made available to the public—and most are—becomes a museum, whether so called or not. Even the university centers of teaching in the subject are almost invariably related to museums either within the university (*e.g.*, the Peabody Museum at Yale, the Museum of Comparative Zoölogy at Harvard, the Walker Museum at Chicago) or outside it (*e.g.*, the American Museum and Columbia University). This close interrelationship between fossils and museums arose almost at the beginning of scientific vertebrate paleontology in America, and quite at the beginning of public museums here. Without ignoring the value of other museum sciences, it may fairly be claimed that vertebrate paleontology has been the most important single factor in the rise and popularization of natural history museums. This is perhaps its greatest contribution to the social, as opposed to the scientific, history of America.

It is said that the first natural history museum in anything like the modern sense was the Ashmolean at Oxford (England) founded in 1667. In the relatively rude colonial days, no such institutions existed in America, although individuals had collections of curiosities, just as the "cabinets" of medieval nobles antedated true scientific museums in Europe. Thus prior to the revolution one Arnold had a collection of birds and insects in Norwalk, Connecticut, but this was a private matter. (See Goode, 1901.)

The beginnings of museums in America can be more definitely seen, although still in an embryonic stage, in the cabinets of several societies. Of these, the cabinet of the American Philosophical

Society was probably the most important. It was a going concern in 1770, and on April 20 of that year it was suggested that the cabinet be encreased by requests to merchants, soldiers, sailors, and others for "new & curious plants, animals & fossils" (*Early Proceedings*). This collection thus definitely antedates that of the Charleston Library Society, which has been considered by some historians (*e.g.*, Bragg, 1923, Smallwood, 1941) as the first museum in America but which was not started until 1773. In any case, and subject again to the difficulty of designating a "first" in the midst of a gradual growth, the first American natural history museum definitely organized as such, public and independent, was apparently Peale's Philadelphia Museum, in which vertebrate paleontology and the American Philosophical Society played the leading parts.[32]

Charles Willson Peale (born April 15, 1741, died February 22, 1827), founder of the first of several Peale museums, was apprenticed as a saddler and followed this trade for a few years, but failed and abandoned it in 1764. He then took up painting, studying in England in 1767–1769, and he is probably best remembered as the most industrious portrayer of George Washington. After the revolution, in which he served with distinction, he gathered together in his house at Third and Lombard streets, Philadelphia, not only a number of portraits painted by himself but also a collection of miscellaneous natural objects including some mastodon remains from Big Bone Lick. In 1785, Peale decided definitely to convert this into a museum and to that end he studied taxidermy and methods of preservation in general, which were very rudimentary at that time. He either invented or was one of the first exponents of some of the most important museum procedures still current, such as the use of arsenic and alum on skins and presenting birds and other animals in habitat groups with painted backgrounds.

The growing Peale museum was organized as a public institution with a board of directors under Thomas Jefferson as president. The directors included Wistar and other prominent members of the Philosophical Society, to which Peale had been elected in 1786. The museum was supported principally by the sale of single admissions and season tickets (to which George Washington subscribed among many other outstanding men of the time). The museum soon outgrew the quarters in Peale's house, and in 1794 it was installed in the hall of the American Philosophical Society—it then occupied the very room in which this paper was presented in 1942. Except for the "S.W. & S.E. corner rooms on the second story," the whole building was leased to Peale, who made himself responsible for its maintenance (including the janitorial care of the Society's reserved rooms) and for the Society's own collection, of which he was already one of the three curators.

Fossil vertebrates were displayed in this museum from the start, indeed it was the acquisition of some mastodon remains in 1785 that gave Peale the idea of starting a museum, but the most important paleontological event connected with it was the collection and mounting of a mastodon skeleton. This was far the first fossil skeleton ever mounted in America (which now has more mounts of the sort than the rest of the world put together) and it was probably the second in the world.[33]

In 1799 bones were found on the farm of John Masten, Orange County, New York. Masten and his neighbors, upwards of a hundred strong, proceeded to excavate with more energy than care, some of the men indulging in spirits to the point of becoming "impatient and unruly," so that it is not surprising to learn that many of the first bones found were broken and some destroyed. C. W. Peale was seeking such an attraction for the museum and when he heard of the incident, in 1801, he visited Masten, purchased such bones as had survived the orgy, and obtained permission to dig for more. The pits had filled with water, which was lowered with great difficulty and expense. "Rich and poor, men, women, and children, all flocked to see the operation, and a swamp always noted as the solitary abode of snakes and frogs, became the active scene of curiosity and bustle (Rembrandt Peale, 1803) (Fig. 16). The work proved so costly that a loan of $500, without interest, was obtained from the Philosophical Society.

The result was most of the skeleton of a mastodon, but the lower jaw and a few other essential

[32] On the lives of C. W. Peale and his sons see especially Sellers, 1934. On activities in the American Philosophical Society, see *Early Proceedings*. On the museums, Colton, 1909. On the Peale mastodons, Rembrandt Peale, 1803, and Warren, 1855. Without citation of authority for each point, the present account is assembled from these and a few other, less important sources.

[33] As far as I can learn, the first was a *Megatherium* skeleton found at Luján (near Buenos Aires), Argentina, sent to Madrid in September, 1789, by the Marquis de Loretto, Viceroy of Buenos Aires, and mounted in the Royal Cabinet, Madrid, by Jean-Baptiste Bru in or about 1795.

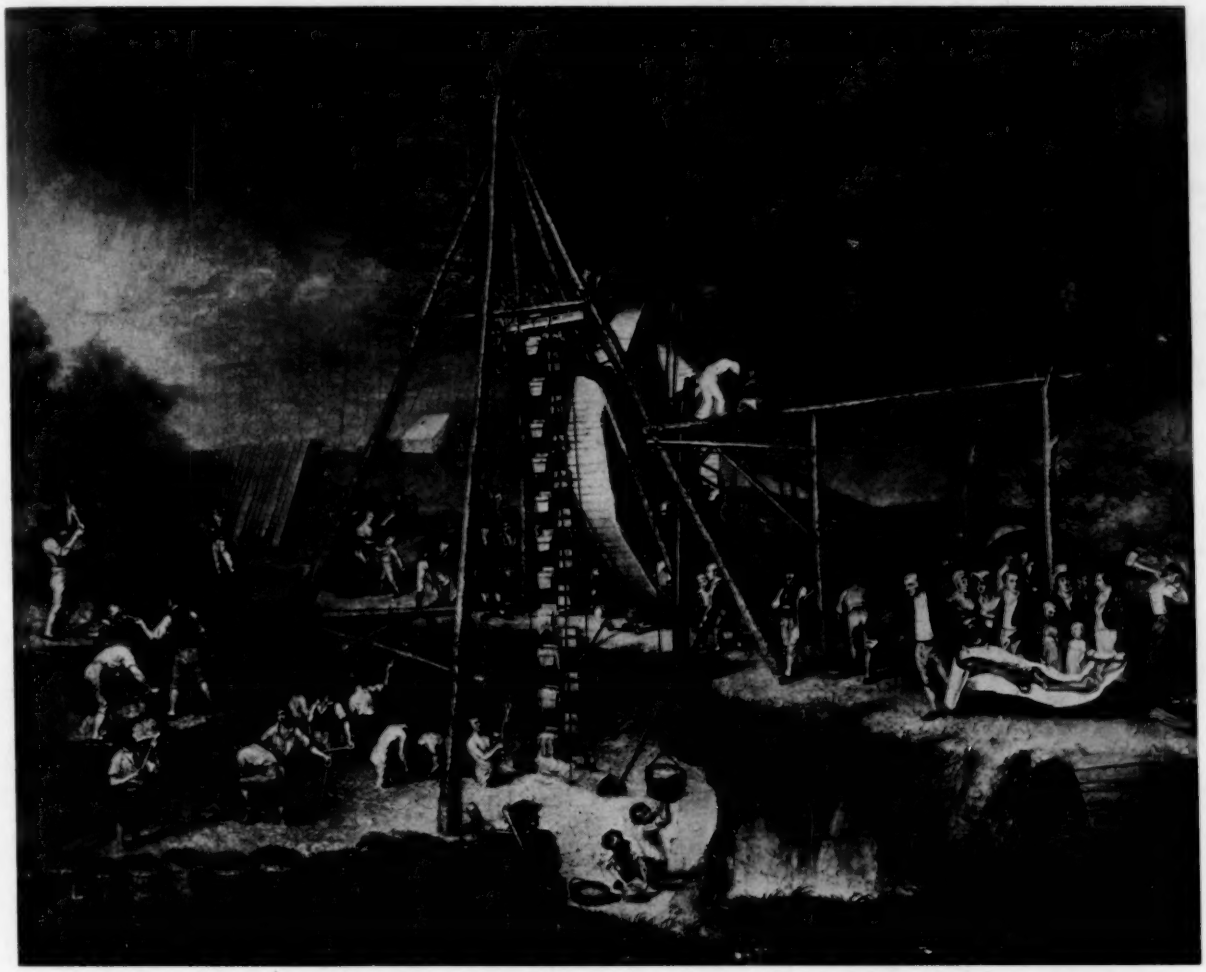

FIG. 16. Excavating the first Peale mastodon. Found near Newburgh, N. Y., in 1799 and excavated in 1801. The skeleton found here was for a time mounted in the Hall of the American Philosophical Society. Of the three men holding the scroll, that to the left is Dr. Charles Willson Peale and the other two are believed to be Titian and Rembrandt Peale. From Osborn, 1923, after a photographic copy in the American Museum of the contemporaneous painting by Rembrandt Peale, belonging to Mrs. Bertha White.

parts were lacking. A second attempt on the farm of Captain J. Barber, where fossils had been found in 1793, eleven miles from Masten's, was disappointing. A final attempt was made on the farm of Peter Millspaw, who had found bones about 1798 some twenty miles west of the Hudson River. Here a skeleton was found, which, although much less complete than the first, included the long-sought lower jaw. "The unconscious woods echoed with repeated huzzas, which could not have been more animated if every tree had participated in the joy. 'Gracious God, what a jaw! how many animals have been crushed beneath it!'[34] was the exclamation of all: a fresh supply of grog went round . . ." (R. Peale, 1803).

It was proposed to complete the first and third skeletons by restoration one from another and one side from the other. The only serious lack was the top of the head and the end of the tail. The work of restoration was done mostly by Rembrandt Peale and the missing bones were carved from wood—the present technique of using plaster casts was available then, and it is not apparent why this much more practical procedure was not followed.

The first and most complete skeleton was mounted in the Philosophical Society's Hall (Fig. 17). It was left there when part of the museum

[34] [A belated survival of the theory that the mastodon was a carnivore.]

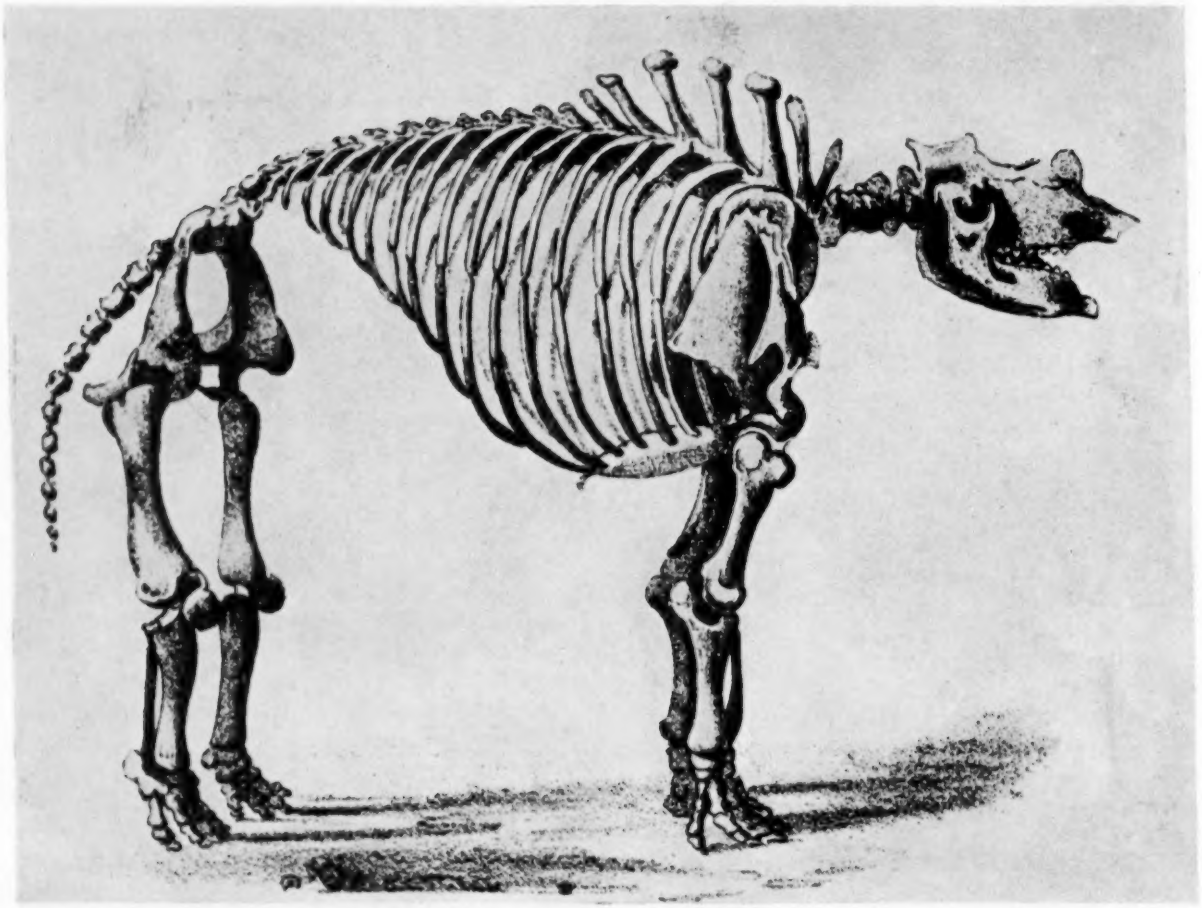

FIG. 17. The Peale mastodon, excavated in 1801. This was the first fossil skeleton to be mounted in America and the second in the world. It stood for a time in the Hall of the American Philosophical Society and later in Independence Hall, Philadelphia. From Warren, 1855.

was moved to Independence Hall in 1802, and was finally removed to the latter place in 1811. In this historic building, occupied until 1828, the mastodon was in the southeast corner of the second floor. The less complete skeleton, the third excavated by the Peales but the second of two restored by them, was taken to England in 1802 by Rembrandt and Rubens Peale and there Rembrandt wrote the family's most significant published contribution to paleontology, his *Historical Disquisition on the Mammoth* (1803). Like the Peales' work in general, this small volume is of great importance for the history of collecting and of museums but made no particular scientific contribution.[35] Rembrandt Peale (born February 22, 1778, while his father was at Valley Forge, died October 3, 1860) took this occasion to study art in Europe and he became probably the best artist in that talented family, although one of the least interested in natural history despite his activity in the mastodon work. The second mastodon skeleton was one of the attractions of Rembrandt Peale's Baltimore Museum, established in 1814 and sold to Barnum in 1845. In 1846 Warren bought the skeleton for his Boston Museum and it was later acquired, along with Warren's

[35] Despite his intimate acquaintance with the subject and his artistic propensities, Rembrandt Peale did not include an illustration of the mastodon skeleton in his book, the (very rare) plate of which has comparative figures of mastodon and elephant heads, teeth, scapulae, and ribs. A rather crude woodcut of the whole skeleton was published by Anderson (in Bewick, 1804), a famous early American illustrator (see Smallwood, 1941, where Anderson's cut is reproduced and references to other accounts of his life and work are given). Probably the best idea of what this historic mount looked like is given by Warren's lithograph (1855), reproduced in this paper, Fig. 17.

own, much more complete mastodon by the American Museum of Natural History, where it is still preserved but is considered too imperfect to warrant being remounted and exhibited.

After C. W. Peale's death his Philadelphia Museum, which had started as a serious and scientific institution despite its commercial aspects, succumbed to the meretricious influences that affected many such exhibits around and after 1840, became a glorified penny arcade, and was eventually sold to P. T. Barnum. The historic first mastodon skeleton was destroyed by fire in one of that showman's dime exhibits.

While popular and public museums passed into the hands of Barnum and his imitators and were, from the point of view of social and educational progress, blasted by their brazen touch, the collections of academies, lyceums, and societies continued to grow and to multiply and formed the soil from which great public museums on a scientific rather than catchpenny basis arose later in the nineteenth century.

The collection of the Philosophical Society was of course retained by it when the Peale collection was removed from the hall. In 1807 and subsequently, the Society's fossil collection was enriched by the munificence of Jefferson and others and became probably the best in America. In 1849 the Society gave up the maintenance of its cabinet and the specimens were deposited in the Academy of Natural Sciences of Philadelphia, where many of them still are.[13] (Details of the specimens still recognizable as having belonged to the Society are given in an appendix to this paper.)

Richard Harlan

"Millions and millions of years have been consumed in establishing the present order of creation; countless myriads of animated beings had appeared and disappeared from the diversified scene, ere yet the wonder, *Man,* was accomplished."—Harlan, 1835.

Vertebrate paleontology did not become a separate vocation during the period now under survey, but among those Americans who pursued various aspects of this science as an avocation, Richard Harlan was the first to do so continuously and over a considerable period, the first who may be said to have had a career as a vertebrate paleontologist. Like most biological scientists of his day, Harlan was trained as a physician. Born in Philadelphia on September 19, 1796, he studied medicine at the University of Pennsylvania. While still a student, he made a voyage to India as ship's surgeon, then returned to Philadelphia and received his M.D. in 1818, the year of Wistar's death. He soon succeeded Wistar as the principal authority on fossil vertebrates in this, the leading center of such studies. He was elected

Fig. 18. Dr. Richard Harlan, first American vertebrate paleontologist, from a bust formerly in the possession of his son, Dr. George Cuvier Harlan. Photograph presented to the American Philosophical Society by Richard Harlan's grand-daughter Mrs. Edward Meguer.

to the American Philosophical Society in 1822 and his first paper, observations on mammoth teeth, was published in 1823. Two years later he published the first volume, on mammals, of a projected complete *Fauna Americana,* and a summary of all known fossil mammals was included. The volume, which was, indeed, a translation and compilation with little originality, had a hostile reception and the proposed second part, on reptiles, was abandoned. In spite of this disappointment, he continued to write frequent short papers on fossils and in 1835 most of these were reprinted with copious additions, in his *Medical and Physical Researches,* ranging over almost the whole field

of natural history as well as medicine, which he continued to practice. In 1838 he removed to New Orleans, whence he sent occasional paleontological notes to the Philosophical Society, the *American Journal of Science*, and others until his death on September 30, 1843. (On Harlan's life, see Fisk, 1932, and his citations.)

Harlan was not a paleontological pioneer in the same sense as Wistar and his predecessors. When he began his studies, Cuvier's work was completed and was eagerly studied by Harlan. The science was established and organized: Harlan had only to practice it. Saying that he did practice it, rather than advance it in any more fundamental way, is not derogatory but is a matter of historical perspective. Harlan's lifetime coincided with a sort of plateau in the rise of vertebrate paleontology, when knowledge of the subject increased greatly in quantity, largely through his efforts, but changed little in quality. Cuvier's theories and methods seemed adequate, indeed complete, and it was not until a vastly greater quantity of information had accumulated that the next great advance became possible. This next advance, to evolutionary, phylogenetic paleontology, came after the end of Harlan's life.

Harlan's most important original contributions to paleontology were in the fields of objective description, nomenclature, and taxonomy. He was the first American to apply Linnaean names to American fossil vertebrates,[36] so that this important aspect of American vertebrate paleontology may be said really to begin with him. A list of genera and species to which he gave technical names is dull, but it best displays this essential part of his work:

Harlan's Name and Date	Current Name or probable Synonymy and Comments
Fish	
Saurocephalus, 1824	*Saurocephalus* Harlan } Described as a reptile, but clearly a fish
S. lanciformis, 1824	*S. lanciformis* Harlan }
Reptiles	
Ichthyosaurus missouriensis, 1834	*Cf. Mosasaurus missouriensis* Harlan
Batrachiosaurus, 1839 }	For *"I." missouriensis*. Technical status uncertain—one of these may be a valid name for an American mosasaur genus
Batrachiotherium, 1839 }	
Crocodilus macrorhyncus, 1834	*Bottosaurus harlani* (v. Meyer, 1832). Von Meyer's name was based on Harlan's study, but was published before Harlan's own name
Ichthyosaurus coniformis, 1835	Uncertain. The type was from Bath or Bristol, England.—The description of an English fossil by an American is significant of the changing status of American Science
Mammals	
Megalonyx, 1825	*Megalonyx*—authorship disputed, usually referred to Jefferson. This reference is certainly erroneous. Harlan may have been the first to use the name in a valid Linnaean form and hence may be its technical author
Megalonyx laqueatus, 1831	Included remains of *Megalonyx jeffersonii* and *Mylodon harlani*. Antedates the latter, but is usually considered a synonym of the former
Orycterotherium, 1842	*Mylodon* Owen, 1840
O. missouriense, 1842	*Mylodon Harlani* Owen, 1840
Basilosaurus, 1834	*Basilosaurus* Harlan. Often, but incorrectly, called *Zeuglodon* Owen, 1839. A cetacean but first described as a reptile
Delphinus calvertensis, 1842	*Lophocetus calvertensis* (Harlan)
Tapirus mastodontoides, 1825	Cooper (1831) thought this a mastodon and later writers have all made it a synonym of *Mammut americanum*, but it is possible that it is really a tapir, in which case the name is valid and antedates any other for our fossil tapirs
Bos bombifrons, 1825	*Boötherium bombifrons* (Harlan)
Bos latifrons, 1825	*Bison latifrons* (Harlan)
Cervus americanus, 1825	*Cervalces scotti* Lydekker, 1898. Harlan's species was valid, but his name was a homonym

[36] The first person to do so was Robert Kerr, a British compiler or hack writer, who included the name *Elephas americanus*, for the American mastodon, in an edition of Linnaeus in 1792.

The list is not long, but it is very impressive when viewed in its historical position. No one else, either American or European, named nearly so many or nearly such a variety of American fossil vertebrates before Leidy. Besides the species named by Harlan, he described and discussed others named by his contemporaries or then still without distinctive names. Practically all of the genera and species described or recognized by Harlan are valid taxonomic units, despite the fact that their names have mostly been changed by retroactive rules of nomenclature far in the future when Harlan wrote and by a narrowing of the scope of such units, also long subsequent to his period.[37]

Harlan's views as to the affinities of these fossil vertebrates were also generally correct, according to the best classifications of recent vertebrates then available. Such errors as calling a mosasaur *Ichthyosaurus,* or a fish (*Saurocephalus*) a reptile, were virtually inevitable in view of the nature of the material available and of the general confusion of the time regarding fossils of lower vertebrates, study of which lagged behind that of fossil mammals. Harlan's most striking failure was in considering *Basilosaurus* a reptile, but even this cannot be imputed either to ignorance or to unscientific procedure. He recognized both mammalian and reptilian characters in the remains of this animal, so totally unlike any mammal that had previously been known, but felt, as anyone might have, that the latter outweighed the former. The correction was not made for five years, and it came from Richard Owen, the greatest comparative anatomist of the age, in London where materials for comparison were incomparably more abundant than those available to Harlan.

Harlan was also the first American student to obtain a thorough grasp of the concept of a succession of fossil faunas, an idea originated largely by European stratigraphers and invertebrate paleontologists and systematically developed for vertebrate paleontology by Cuvier. This helped to make Harlan's studies modern in tone and led to a really orderly science of vertebrate paleontology in this country. The number of pre-Pleistocene fossils available to Harlan was so small and their occurrences were so spotty, both stratigraphically and zoologically, that no picture of a general, still less of a gradual, sequence could possibly have been derived from them. What fossils he did have were nevertheless placed in the geological column with almost perfect correctness although of course with far less precision than has subsequently become possible.

Harlan's most important broadly theoretical or philosophical studies are given in his papers "On the Affiliation of the Natural Sciences" (Harlan, 1835, pp. xiii–xxxix) and "On the Successive Formations of Organized Beings" (Harlan, 1835, pp. 232–252). The first of these essays beautifully expresses the essential interrelationship of the whole of creation and the necessary interdependence of all branches of the study of nature and of man. It emphasizes the constant flux of nature, the profound changes that have occurred in what theologians still considered the eternally static face of the earth and species of its inhabitants.

Harlan was thoroughly familiar with the theory of organic evolution as expressed by Erasmus Darwin, Lamarck, and especially Virey. Although Virey was the least important of these precursors, he did give a clear and on the whole surprisingly modern expression of the evolutionary principle. Harlan gave an adequate summary of Virey's views and a briefer abstract of Lamarck's. For religious authority he cared little: "All theological considerations aside, we would be inclined to take the facts as they are." Beyond this, Harlan's opinions were somewhat equivocal. Species are absolutely immutable: "Between species and species nature has drawn a line of separation, which time cannot change nor the sophistry of man obliterate." But if species do not change, as units, they have nevertheless repeatedly been replaced by other species. If evolution is true, Harlan saw that paleontology should give the final and direct evidence for it. Such evidence went as far as to demonstrate this replacement, as Cuvier had shown. Even this was enough to stamp its supporters, like Harlan, as infidels, but he aptly replied that when those who resort to the "puerile practice of calling names . . . interfere in questions of science, it is always for the purpose of suppressing, not for *promoting* knowledge."

[37] Aside from his descriptions and discussions of others, Harlan was the first to designate twelve fossil vertebrate species (or analogous groups). How far from negligible this was, can be judged by the fact that Cuvier, whose career overlapped that of Harlan, knew fewer than eighty species of fossil vertebrates in all, gathered from four continents and described by numerous authors. The description of one hundred new species now is hardly more remarkable than the description of one at that time.

Where, then, did the new species come from? Early nineteenth century paleontology gave no answer. Every known fossil species appeared full-fledged and remained as such until it disappeared. It was therefore a proper scientific inference for Cuvier and, following him, Harlan to believe that species were immutable. The tendency of the evidence actually at hand was correctly interpreted: the failure lay in lack of comprehension of the great inadequacy of that evidence to prove the conclusion and in lack of vision to see that the filling in of the gaps could, as it later did, prove the opposite. Harlan had no clear answer to the problem of the origin of species. He avoided direct statement of divine creation and, somewhat obscurely and uncertainly, he suggested that species arose by spontaneous generation. Each variety of earth has its characteristic plants and animals. When the flux of geological conditions changed the earth, perhaps the new sort of earth produced new species of life —a daring and, to us, curious hypothesis but one as consistent with the facts then known as was the theory of evolution in its more primitive forms.

Finally Harlan attacked the idea of the gradual perfecting of organisms, an idea then already involved in evolutionary philosophy and still, a century later, a topic of philosophic discussion and considerable misunderstanding. "Throughout the whole animal creation, no one being is imperfect; but . . . each is equally finished and perfectly adapted to perform its part in the sphere in which its destiny had placed it." This too sweeping and admittedly somewhat crude perception of the fact of adaptation on all levels of organization was accompanied by Harlan's equally crude but equally prophetic belief that new environmental conditions would be accompanied by the emergence of new species adapted to the new conditions. Here are all the essential elements of advanced evolutionary theory except one: the actual transmutation of one species to another. The fossils known in Harlan's day were simply too few to support this final step, or to reveal to him where his thought was tending.

Harlan's Contemporaries

"Je termine donc ici cette édition . . . laissant à mes successeurs à cultiver un champ que je n'ai fait qu'ouvrir, et qui bien certainement leur donnera encore des moissons plus riches que toutes celles que j'ai pu recueiller."—Cuvier, in the closing passage of the last edition of his great *Ossemens Fossiles* (1836).

Harlan was so much the most important American contributor to vertebrate paleontology in the early nineteenth century that he has been treated at some length. Space will not permit so full a discussion of the work of any other in the same period, or even a list of the many who then had something to say on the subject. Most of these may, indeed, be dismissed either as inexperienced persons who created more confusion than clarity, *e.g.* James Pedder (see Simpson, 1936), or as men who were, indeed, able scientists but who reviewed knowledge of fossil vetrebrates without adding significantly to it, *e.g.* Benjamin Smith Barton.

Vertebrate paleontology then had two main centers. In Philadelphia the American Philosophical Society and the Academy of Natural Sciences had as active workers, besides Harlan, such figures as Godman, Hays, Horner, and Morton. In New York the Lyceum of Natural History included not only its principal founder, Mitchill, but also Cooper and De Kay. In New England Hitchcock and the Redfields did important work without being so closely associated with a particular scientific group, although Hitchcock was a member of the American Philosophical Society, to which belonged a strong majority of those then concerned with fossil bones.

John Davidson Godman (December 20, 1794–April 17, 1830, see Miller, 1931) spent his short life in many places, Annapolis, Baltimore, Cincinnati, Philadelphia, New York, the West Indies, but the years most important for science were those in Philadelphia, where as Rembrandt Peale's son-in-law and, from his thirtieth year, a member of the Philosophical Society he was part of a circle of naturalists among whom fossil bones were a constant subject of discussion. His great work was the *American Natural History* in three volumes, 1825–1828, and several species of fossils were mentioned in this. His original contribution to paleontology was the description (to the Philosophical Society) of *Tetracaulodon mastodontoideum,* a purported new genus and species differing from *Mammut americanum* (or *Mastodon giganteum* as it was then usually called) in having small tusks in the lower jaw. This started an occasionally acrimonious controversy in which Godman, Hays, and others maintained the taxonomic validity of this distinction, while Harlan and others insisted that the lower tusks were merely juvenile or anomalous. Time has sustained Harlan's contention as regards the speci-

mens then in question, although the lower tusks were normal adult characters in ancestral mastodons then unknown and their taxonomic significance was a reasonable hypothesis when advanced by Godman.

This skirmish became part of a feud between Harlan and Hays that was less prolonged but scarcely less bitter than that between Cope and Marsh later in the century. Thus early appeared a tendency toward personal rivalry in vertebrate paleontology of which, happily, only the last faint trace still remains as a blot on the science, or rather on its practitioners. Isaac Hays (July 5, 1796–April 13, 1879, see McCrae, 1932) was of almost exactly the same age as Harlan, but his incursions into paleontology were met by the latter with the scorn of the master for the neophyte —an attitude somewhat justified by the facts, however unjustified as a reaction to those facts. Indeed in this field Hays did lack both the experience and the judgment of Harlan. His published paleontological studies were almost confined to the elucidation of the differences between two supposed genera and seven supposed species all of which are now agreed to be merely individual variants of *Mammut americanum,* as Harlan claimed at the time. It was the beginning of the struggle between the splitters or species-makers and the lumpers or synthesists.[38]

In spite of this failing, Hays' paleontological activities in the Philosophical Society, to which he was elected in 1830, were many and beneficial. He was for years the member most concerned with building up the Society's collection of bones, with demonstrating the peculiarities and diversity of these, and with communicating paleontological news to the other members. As the disagreement of the two men developed, Harlan tended to use the Academy as his stronghold, although never wholly withdrawing from the Society. Aside from his paleontological hobby—it was little more —Hays had a very distinguished career as a pioneer ophthalmologist and medical editor.

Horner and Morton may be considered as the respective successors of Wistar and Harlan. Like their predecessors, as well as Godman and Hays, their training was medical. William Edwards Horner (June 3, 1793–March 13, 1853, see Miller, 1932) became Wistar's prosector and eventually (after J. S. Dorsey) his successor at the University of Pennsylvania. The medical school of that university may be said to have been his great life work, but he also wrote the first American text on pathology and a treatise on human anatomy. He was a friend of Leidy, with whom he visited Europe in 1848, and he probably had a part in turning Leidy's attention to fossil bones, which in itself would give him a place in the history of vertebrate paleontology. Aside from this, he was active with Hays in promoting the subject in the Philosophical Society. He also made a detailed examination of Koch's collection, which will be briefly mentioned on a later page, and made sober corrections of the extravagant nonsense publicized by that dubious individual—a feat of no mean difficulty and importance.

Samuel George Morton (January 26, 1799–May 15, 1851, see Fisk, 1934) was one of the truly great figures in the history of American paleontology, but his original contributions were more extensive in the field of invertebrate paleontology than in that here under review. He was extraordinarily versatile even for a period when scientific versatility was the rule and he also made important pioneer studies of medicine, geology, zoology, and physical anthropology. His geological specialty was the Cretaceous and his most important publication was the *Synopsis of the Organic Remains of the Cretaceous Group of the United States* (1834). Along with more fundamental studies of fossil shells, this and his other works on related subjects summarized the few known Cretaceous vertebrates. The most important innovation was inclusion of several sorts of fossil shark teeth, usually identified by Agassiz, who was considerably influenced by Morton. (Aside from this, Agassiz's American career falls beyond the limit set for this review and is not further considered.) In general, Morton continued the same sort of systematic paleontology that had been initiated by Harlan, but accom-

[38] Hays did, however, describe one species that was unquestionably distinct, *"Mastodon" chapmani,* now referred (by Osborn) to *Stegomastodon.* This was based on a single broken molar of unknown origin and it remains a paleontological mystery to this day. No other tooth exactly like it has ever been found, but it seems to be most closely allied to forms common in western United States and in South America but very rare or absent in the eastern states. Osborn several times listed Hays' species as from Nebraska and went so far as to claim that it was the first fossil ever found in the western territories (1931, p. 32)—a claim that could not be supported even were this origin demonstrated. Since the locality is really unknown and might have been almost anywhere from Canada to Patagonia, its bearing on the geographic extension of paleontological discovery seems to me too dubious to merit emphasis or further mention.

plished less as regards vertebrates because of the paucity of Cretaceous material.

Morton became a member of the Academy of Natural Sciences of Philadelphia when 21 years of age and continued there all his life, being president of the Academy when he died. It was largely his doing that the Academy became the great center for vertebrate paleontology in the late 1840's and continued pre-eminent for several decades thereafter. He was also a member of the American Philosophical Society (elected in 1828) and was instrumental in the movement by which the Academy became the local institution for prosecuting research and the Society a national organization for promoting science and diffusing its results. This development of functions is reflected in the transfer of the Society's fossil collections to the Academy in 1849, during Morton's tenure.

In New York, the Lyceum of Natural History, founded by Mitchill in 1817, could claim no such dignified antiquity as the Philosophical Society in Philadelphia, but it was an active younger brother and soon concerned itself with most of the subjects discussed at the Society, including vertebrate paleontology. Its most important early success was the acquisition of remains from Big Bone Lick, some of them collected by William Cooper, who wrote one of the best and most complete descriptions of the lick and its fossil fauna (Cooper, 1831).

Samuel Latham Mitchill (August 20, 1764–September 7, 1831) was considered "a chaos of knowledge," "remembered more for the goodness of his heart than the strength of his head" (Newell, 1934). He practiced medicine, saw long service in the national Congress, and at various times was professor of natural history, chemistry, agriculture, botany, and materia medica. Except for some mineralogical studies, his researches consisted chiefly of compilations, such as a list of known American fossils in his translation of Cuvier's *Theory of the Earth* (1818). He was, however, the first to announce the occurrence of *Megatherium* in North America (1824) and of walrus bones as far south as Virginia (1828).

James Ellsworth De Kay (October 12, 1792–November 21, 1851, see Shaw, 1930) might be called the Harlan of the Lyceum except that his paleontological work was much less extensive. He was also a physician and an ardent naturalist, although his main interests and associations seem to have been more literary than scientific. Like Harlan, he described some fossils very much in the manner of today, and he also summarized them in more general works such as the *Zoology of New York*, for which he is best remembered.

In spite of the interest and activity centering around these worthies of the Lyceum, greater importance attaches to the work of Hitchcock and Redfield. Hitchcock (May 24, 1793–February 27, 1864, see Merrill, 1932), one of the most famous figures of American geology, was educated for and entered the ministry, but while studying theology at Yale he fell under the influence of Silliman and became an ardent geologist as well. So little did the two fields of endeavor conflict for him that he later wrote a treatise on the religion of geology and was for a time Professor of Geology and Natural Theology. He initiated the great geological and paleontological tradition of Amherst College, where he taught for many years and of which he was for a time president. As director of the Massachusetts geological survey, 1830–1833 and 1837–1841, he became particularly concerned with the fossils of the Triassic sediments of the valley of the Connecticut River. Solomon Ellsworth, Jr., of East Windsor, Conn., had found some bones in the red sandstones in 1818. Nathan Smith (1820) recorded the discovery, considering the bones as possibly human. They are still preserved at Yale University and were identified much later (Lull, 1915) as belonging to a primitive dinosaur, *Anchisaurus*. In 1841 Hitchcock described and figured some natural bone casts from Ellington, Conn., but these could not then be identified. It was not until 1858 that Hitchcock had a specimen sufficiently complete to demonstrate its reptilian nature, and the dinosaurian affinities of these bones were finally demonstrated by Cope in 1870.

In the meantime the footprints in these strata, so much more abundant than bones, had long excited popular conjecture and in 1836 Hitchcock produced a first scientific study of them. At that time he believed that all the tracks were those of birds, and from the start he held that the tracks could be so classified as to produce a classification of the animals that made them—a taxonomic and nomenclatural problem on which general agreement still has not been reached. In the first paper, the footprints were placed in seven species (and four additional varieties) regarding which Hitchcock made a penetrating comment: "When I speak of species here, I mean species in oryctol-

ogy,[39] not in ornithology. And I doubt not, that in perhaps every instance, what I call a species in the former science, would be a genus in the latter." Here, again, Hitchcock raised a question that is just beginning to find an approximate answer, more than a century later. In 1837 Hitchcock divided the tracks into many-footed (insects or other invertebrates), four-footed and two-footed. Among the last, he recognized some as more reptile-like and some as more bird-like, without asserting definitely that they were made by reptiles or by birds. Thus at this early date he rejected previous positive (but erroneous) statements that birds made the tracks and he suggested that some, at least, could be reptilian, which has since proved to be true for almost all of them. Hitchcock devoted some thirty years to gathering and studying these tracks, mostly after 1842, and he was chiefly responsible for the superb collection of them at Amherst College.

—As a digression, this exception calls attention to the small part that the early colleges and universities play in this history. In later periods, educational institutions were major factors in the development of vertebrate paleontology, but in the earlier years there were no professors of paleontology, no separate courses in the subject, and almost no research on it in connection with teaching. The University of Pennsylvania was the first to have competent students of fossil vertebrates on its faculty, Wistar and his successors, but their research in this field was connected with the Philosophical Society and the Academy, not the University. There was a mastodon tooth at Yale College as early as 1786 (see Parsons, 1793), probably the first vertebrate fossil in an American college collection, and Silliman mentioned fossil vertebrates in his geology course, but there was no professionally competent student of such remains at Yale until Marsh (1865). Amherst was probably the first college to possess any considerable collection of vertebrate fossils under the study and care of a qualified faculty member.—

William C.[40] Redfield (March 26, 1789–February 12, 1857, see Humphreys, 1935) was a student of Triassic fossils, like Hitchcock, but mainly of the fish of that period. He was the first American specialist on fossil fish. Such studies were an avocation for him as they were for all paleontologists of the time, but he was, with Hitchcock, an exception to the rule that paleontologists were primarily physicians. Like the elder Peale, he was apprenticed as a saddler and followed that trade for some time. In later life he became a transportation promoter and he established the first barge line on the Hudson. His most remarkable scientific accomplishment was his proposal (first published in 1831) of the rotary theory of hurricanes, based on observations of the great New England storm of September 3, 1821. This exposed him to attack and opprobrium at the time, but history has vindicated him and honors him as a great pioneer in meteorology. He was, incidentally, the first president of the American Association for the Advancement of Science (1848).

Redfield's most important paleontological work consisted of seven short papers on Triassic fishes, most of them published in the *American Journal of Science*. Only three of these papers were written prior to 1842, but even at that date Redfield had established the presence of these ancient fishes in Massachusetts, Connecticut, New Jersey, and Virginia. He named one new genus and seven new species, and he described and discussed several others. With one minor exception, his taxonomic units are all accepted as valid by twentieth century revisers, despite the usual, inevitable change in nomenclature—a remarkable record. His ideas of the affinities and of the stratigraphic relationships of these fish were also generally correct, within the established classificatory framework of that time.[41]

Almost all the really important additions to knowledge of American fossil vertebrates during this period were made by men who have now been mentioned. Isolated exceptions are provided by William Williams Mather of Ohio, who named the first American mammoth to be distinguished from the Siberian prototype (*"Elephas" jacksoni*, 1838) and John W. Foster, also then working in Ohio, who named the first discovered American fossil rodent (*Castoroides*, 1838). The latter discovery was, however, made, or at least was first reported, in 1837 by Samuel Prescott Hildreth, who gave a figure of *Castoroides* remains without naming them.[42]

[39] ["Oryctology" was then a usual name for "fossil zoölogy," or what we now call "paleontology."]

[40] One of the interesting peculiarities of this remarkable man was that his middle initial was only C., and did not stand for any name.

[41] The first paper on these subjects by a Redfield, in 1837, was in fact written by W. C. Redfield's son, John Howard, but he did not follow this up and the elder Redfield is the important figure.

[42] The announcement was anonymous and is usually attributed to Foster, but Hay, 1929, credits it to Hildreth. Hildreth also proposed the name *Ovis mammillaris* for a supposed fossil sheep, but this was probably based on remains of a domestic sheep.

One more personality of this period demands special attention: Albert Koch, an energetic and colorful man of no scientific ability. His history and writings are so picturesque and, in some respects, amusing that they still attract attention and have been overemphasized. The danger of this undue emphasis is that it tends to give the impression that Koch's work was typical of the time or outstanding in it, a serious distortion of history and a grave injustice to the able and honest American scientists who had been doing excellent work in vertebrate paleontology for more than a generation before Koch appeared on the scene.

Koch's data and publications have little scientific value and hardly merit mention here unless possibly as comic relief. The incident does, however, involve two points of some historical importance. First, Koch did gather large collections, parts of which eventually received more sober treatment. Second, the popular acclaim and private profit of his transactions are revealing as regards the social environment of American science toward the middle of the nineteenth century. Through the efforts of the great research museums, of scientific educators, and of astute but conscientious popularizers, the present generation of vertebrate paleontologists has inherited an environment in which paleontology is a relatively familiar and tolerated subject. There is no wide appreciation of its more abstruse aspects, but the results as presented to the public are known to be the outcome of serious research and are generally rejected if their origin is not respectable.[43] In Koch's day even the most rudimentary conception of paleontology was confined to a handful of learned men and the populace as a whole accepted his astonishingly unscientific presentations with greater acclaim than was granted to any of the results of sober work.

Although he gave many details regarding some of his discoveries, Koch's reports inspire no confidence and it is often dubious where and under what circumstances individual specimens were acquired. According to his account, his first "Missourium" bones were found "in May, 1839, on the premises of Capt. Palmer, 22 miles south of St. Louis, in Jefferson county, Missouri, in the immediate vicinity of Sulphur Springs, on Little Rock creek" (Koch, 1839). The large bones all clearly belonged to the common American mastodon, already a very familiar animal to paleontologists, but they were crushed and this, together with ignorant reconstruction and fantastic articulation, enabled Koch to point out numerous supposed differences and to claim that he had found two new gigantic animals, one an ally of the mastodon and the other entirely new and to be dubbed the *Missourium*.

Koch had already, in 1838, found bones of a reputedly clawed mastodon in Gasconade County, Missouri, and he later, in 1840 or 1841, found still more in Benton County, where the occurrence of large fossils had been known since at least 1806.[44] From bones acquired at one or all of these places he then constructed a skeleton 32 feet in length and 15 in height with a skull 6 feet long circled round about by tusks 21 feet from tip to tip along the curvature and 15 feet straight across the head. Then lo! the *Missourium* stood revealed as none other than the Leviathan of holy writ, a proposition almost self-evident and sufficiently demonstrated to impious doubters in a point by point comparison of the skeleton with the description given in Job (Koch, 1841 and subsequently). This wonderful *Leviathan Missourii* skeleton was eventually acquired by the British Museum, where it was reduced to the prosaic dimensions of *Mammut americanum* and is still on display (or was before the bombing of the Museum).

Koch's subsequent career as a collector of *Basilosaurus* bones in the south and as a showman exhibiting his marvels in America and Europe happily lies beyond the period set for this historical review.

Discovery of Western Fossils

"found a fish back bone pitrified also the h^{d}. just below the Is^{d}. on the top of a hill Situated on the L.S."—Log of the Lewis and Clark Expedition, Sept. 10, 1804.

It is generally stated that the discovery of vertebrate fossils in the western plains and mountains dates from 1846 when Dr. Hiram Prout, of St. Louis, made known a titanothere tooth from what are now called the Big Badlands of South Dakota. The importance of this discovery was exception-

[43] For example even a frankly and intentionally fantastic recent motion picture made the gesture of having a reputable paleontologist vouch for its cartoons of prehistoric animals.

[44] Because his first publication is extremely rare and because his later pamphlets were not clear in this matter, it is often assumed that the *Missourium* was from Benton County or sometimes that it was from Gasconade County. If such an artificial monstrosity can be said to have a type locality, Jefferson County is typical for the *Missourium*.

ally great and it is true that it, as nearly as any one incident, marks the beginning of the intensive search for western fossils that is still unabated, nearly a century later, and that has been the principal factor in the pre-eminence of this science in America. Nevertheless this discovery had antecedents and it was far from marking the absolute beginning of the discovery of western fossils or of their scientific study. When Prout's *"Palaeotherium"* was found, vertebrate remains from both the near and the far West were already known in considerable variety, and such finds had been made sporadically for more than forty years (Fig. 19).

In 1782 (or perhaps 1781), Jefferson wrote, "A Mr. Stanley, taken prisoner by the Indians near the mouth of the Tanissee [Tennessee River], relates, that after being transferred through several tribes, from one to another, he was at length carried over the mountains west of the Missouri [Rocky Mountains?] to a river which runs westwardly: that these bones [fossil bones, presumed by Jefferson to be of the same kind as those found at Big Bone Lick] abounded there, and that the natives described to him the animal to which they

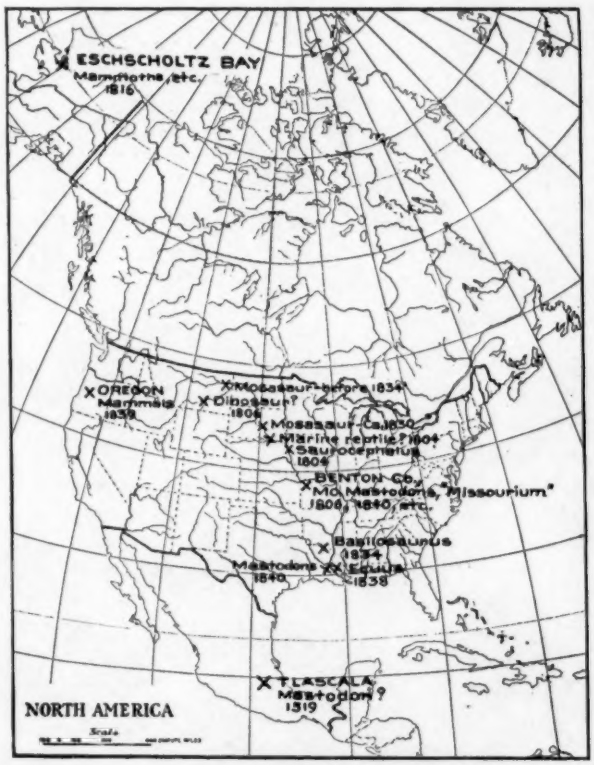

FIG. 19. Localities and dates of some discoveries of fossil vertebrates previous to 1842 in some of the more western and outlying parts of North America.

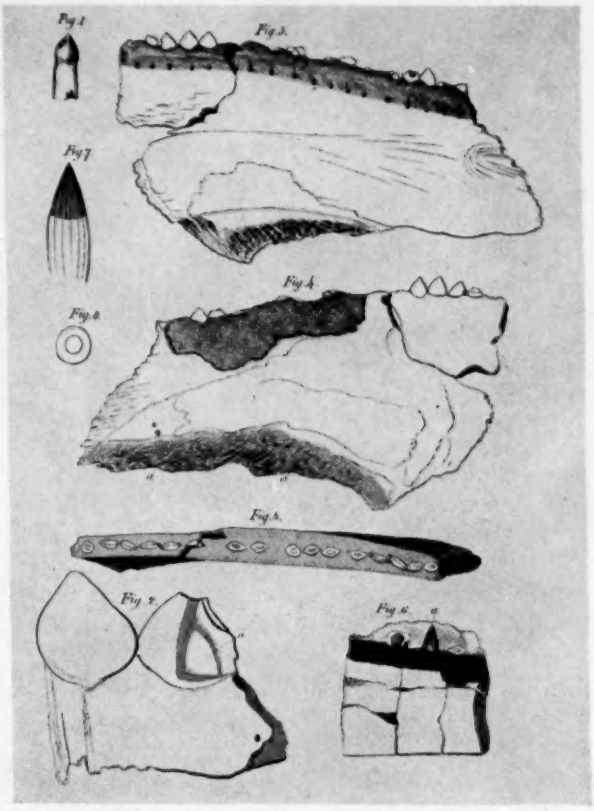

FIG. 20. Type of *Saurocephalus lanciformis* Harlan, 1824, found by Lewis and Clark in 1804, one of the first western fossil discoveries. Drawings by Titian Peale (engraved by Lawson), from Harlan, 1835.

belonged as still existing in the northern parts of their country; from which description he judged it to be an elephant" (Jefferson, 1825, p. 59). If Mr. Stanley's statement was authentic, this is the earliest known reference to western fossil bones, but it cannot be called a discovery.

It was Jefferson who was chiefly responsible for sending out the famous Lewis and Clark expedition, and the American Philosophical Society was also involved in the origin of this plan and concerned with its execution. This party made the first explicit records of western fossil localities and brought back the first specimens.

Their first discovery was the fossil fish that Harlan (1824, 1835) described as *Saurocephalus lanciformis* (Fig. 20). He gave its origin as follows: "About sixteen years ago,[45] there was de-

[45] *I.e.*, about 1808. The necessary correction for lapse of time was not made in Harlan's publication of 1835, which still referred to this as "about sixteen years ago" (G. G. S., note).

posited by Lewis and Clark, in the cabinet of the American Philosophical Society, a fossil organic remain of some unknown marine animal. During the expedition of these gentlemen up the river Missouri, in the year 1804, this specimen was found in a cavern situate a few miles south of the river, near a creek named Soldier's River. . . . A few miles down the river, at Council Bluff, there are hills of considerable size. . . ."

The specimen and its original label (Fig. 21) are now in the Academy of Natural Sciences of Philadelphia, catalogued as No. 5516. The handwriting of the label is not Clark's but is almost surely Lewis's. It reads:

No 9

petrefed jaw bone of a fish or some other ana-
-mal found in a cavern (some distance) a few
miles distant from the Missouri. (the) S. side of
the river—6th of August 1804.

(The words in parentheses have been crossed out in the original.)

Although less explicit than Harlan's statement, this label is in agreement with it, because August 6 was the date on which the party passed the mouth of Soldier's River. The finding of the fossil is not mentioned in the journals of the expedition. Soldier's River is still so called and it flows southward into the Missouri in Iowa, above Council Bluffs. Harlan's statement and the apparent agreement of Lewis's label that the fossil was found south of the Missouri near Soldier's River are confusing, first, because the Missouri is here flowing almost due south and, second, because if either side could be called south of the river it would be the (south-) west side and Soldier's River is on the other side. A possible explanation is that Lewis meant "starboard side," *i.e.* east, not "south side," when he wrote "S. side." Clark habitually called the right side, oriented by the direction of travel, the "S. side" or "S.S.," although it is true that Lewis sometimes used "N." and "S." for north and south and "L." and "R." for left (larboard) and right (starboard) instead of "L." and "S." respectively. In any case the specimen should be either from what is now Harrison County, Iowa, or from what is now Washington County, Nebraska. The specimen, itself, apparently indicates an Upper Cretaceous marine deposit, which is also puzzling because no such deposit is noted in this vicinity on the geologic maps available to me. Its occurrence in a cavern is also somewhat anomalous and can be explained only by occurrence in the wall rock, not in a cave deposit, properly speaking.

This is the only Lewis and Clark vertebrate fossil that has been technically described and the only one that I have been able to locate in surviving collections, but Clark recorded two other discoveries of remarkable interest in his journal, and these were announced in the earliest authentic account of the expedition (Allen edition, 1814). That publication is important as putting the finds on definite and public record at the time, but the incidents are more fully given in the original journal (Thwaites edition), which will be quoted.

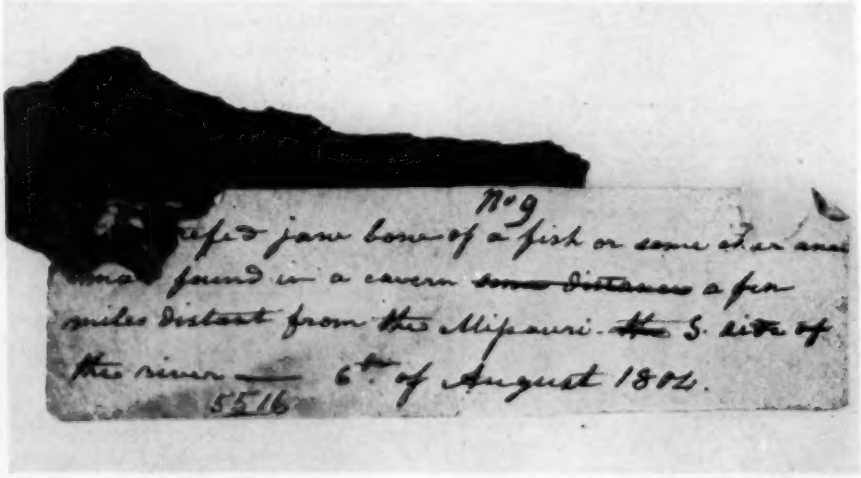

FIG. 21. Original field label of *Saurocephalus lanciformis*, in Lewis's handwriting, pasted on the specimen. Photograph from the Academy of Natural Sciences of Philadelphia.

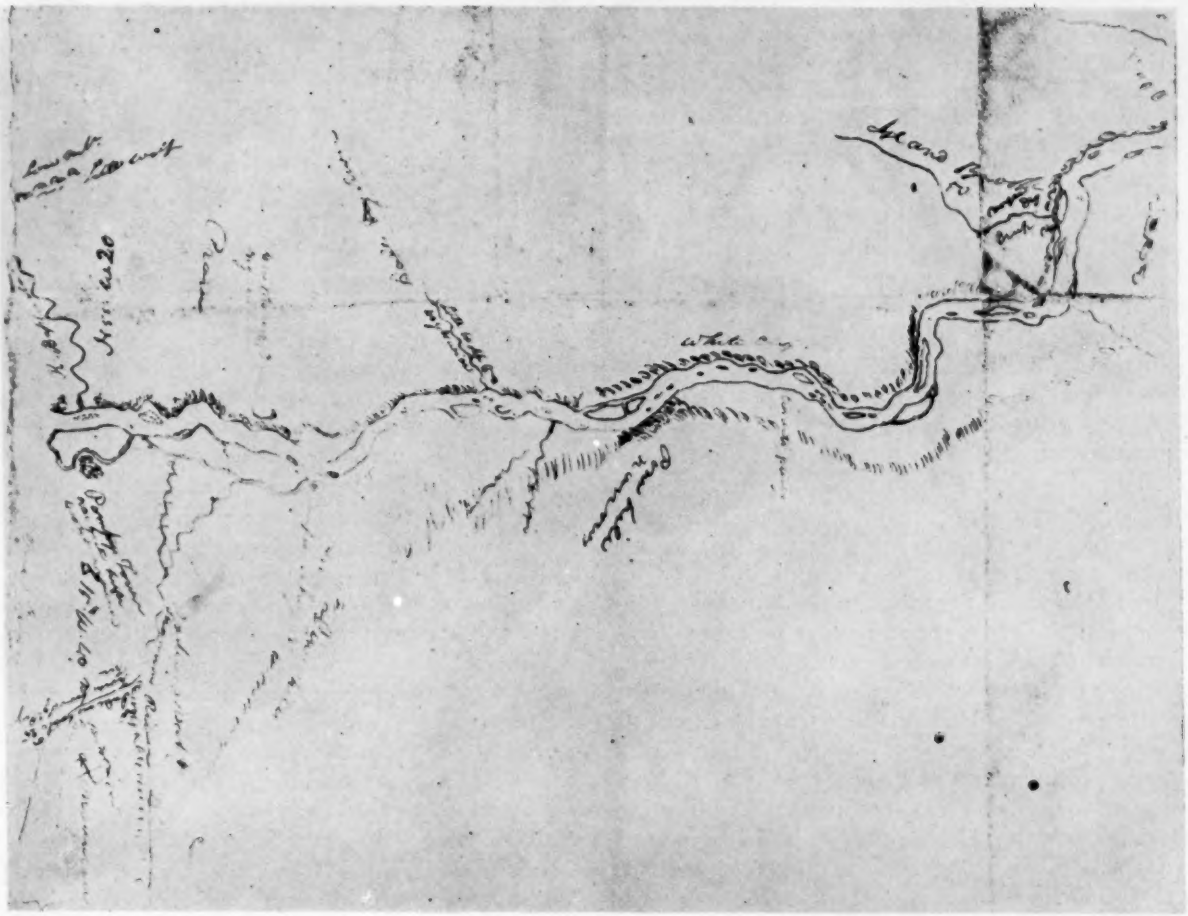

FIG. 22. Clark's manuscript map of part of the Yellowstone River. Pompey's Pillar ("Tower" on the map) is at the left margin of this sheet. The place marked "White Clifts" north of the river to the right of the middle of the figure is where Clark found a fossil bone in 1806, probably the first discovery of a western dinosaur. Original in the American Philosophical Society, published as Plate 51 of the atlas of Thwaites, 1905.

Clark's journal for Monday, Sept. 10, 1804:

. . . Passed the lower point of an Island covered with red Ceeder Situated in a bend on the L. S. [larboard, south, side] this Island is about 2 Miles in length below this on a hill on the L.S. we found the back bone of a fish, 45 feet long tapering to the tale, Some teeth &c. those joints were Seperated and all Petrefied.

The published version of 1814 adds that fragments were collected and sent to Washington, but nothing has been learned of the fate of these.

The manuscript map (Thwaites edition, atlas, map 5[46]) does not designate the fossil locality but shows the island with red "Ceeder" as "I. au Cedar." The locality is now in South Dakota, near the Nebraska line, between Niobrara and Wheeler. Upper Cretaceous marine beds occur in this vicinity and, although the specimen may have been a fish (exaggerated as to size), it was probably a mosasaur or plesiosaur.

Still more interesting is this entry in Clark's journal for Friday, July 25, 1806:

dureing the time the men were getting the two big horns [sheep, shot for food] which I had killed to the river I employed my self in getting pieces of the rib of a fish which was Semented within the face of the rock this rib is (about 3) inches in Secumpherence about the middle it is 3 feet in length tho a part of the end appears to have been broken off (the fallen rock is near the water—the face of the rock where rib is is perpend[icula]r-4i[nche]s lengthwise,

[46] The editor's legend to this map is "Route from May 21—about June 20, 1804," but there is some mistake, because the map shows the camping place of Sept. 9–10 as well as Cedar Island, passed on Sept. 10.

a little barb projects I have several pieces of this rib the bone is neither decayed nor petrified but very rotten. the part which I could not get out may be seen, it is about 6 or 7 Miles below Pompys Tower in the face of the Lar[boar]d [north] Clift about 20 feet above the water.

The manuscript map (Thwaites edition, atlas, map 51; Fig. 22 of this paper) shows the locality, marked "White Clifts," without mention of the bone. Pompey's Pillar (as it was called in subsequent publication) is on the south side of the Yellowstone River below Billings, Montana, and still bears that name. Hell Creek (latest Cretaceous) beds outcrop along the river near here, and the "fish" rib doubtless belonged to one of the dinosaurs for which that formation is now famous. Had Lewis's clue been followed up, investigation of the western dinosaur fields would have been advanced by two generations—their discovery, in a scientific sense, was not made until 1877.

In 1834 Harlan described to the American Philosophical Society a specimen that he called *"Ichthyosaurus" Missouriensis,* actually a mosasaur, although the error was then a natural one and was soon corrected by Harlan (1835) to the extent of saying that the specimen is quite distinct from known ichthyosaurs and should be placed in a separate genus. This specimen was given to Harlan by Major N. A. Ware, who had obtained it from a trader. The label, as quoted by Harlan, was as follows.

> A trader from the Rocky Mountains, on his return, near the Yellow stone knobs or hills, observed, in a rock, the skeleton of an alligator—animal, about seventy feet in length; he broke off the point of the jaw as it projected, and gave it to me [presumably to Ware]. He said that the head part appeared to be about three or four feet long.

Elsewhere in his description, without citation of authority, Harlan spoke of the locality as on the banks of the Missouri near its junction with the Yellowstone. "Near" must here be used in a very relative sense, because the specimen is certainly from Upper Cretaceous marine rocks and such strata do not appear in the valley of the Missouri for more than fifty miles upstream from its junction with the Yellowstone or downstream for several hundred miles.

A similar but more important find was made at about the same time or earlier, probably around 1830 and surely before 1835, although the description of it was not published until 1845. This was an excellent skull and mandible with considerable parts of the skeleton of a mosasaur, named *Mosasaurus maximilianus* by Goldfuss (1845), who stated that Prince Maximilian zu Wied had obtained it during his travels in interior North America. "Sie waren von dem Major O'Fallon, ehemaligen Agenten für die Indianischen Nationen des oberen Missouri, in der Gegend des Big-Bend, einer grossen Krümmung des Missouri, zwischen Fort Lookout und Fort Pierre, gefunden, nach St. Louis gebracht und in dem Garten des damaligen Besitzers [O'Fallon's] niedergelegt worden."

The specimen is clearly from Upper Cretaceous marine beds, perhaps from the Pierre shales, and it came from near the type locality of that formation, in what is now South Dakota (not from Nebraska or Missouri, as sometimes stated).

In 1835 (p. 284) Harlan noted that the Prince de Wied, on passing through Philadelphia, told him of this acquisition and suggested that the animal might be the same as Harlan's *"Ichthyosaurus" missouriensis*—a conclusion that we now know to have been at least approximately correct.

Various discoveries before 1842 west of but near the Mississippi are not properly western in the present sense, but discoveries in western Missouri, not far from the Kansas line, may be mentioned. The occurrence of fossil mammals in Benton County, Missouri, was known at least as early as 1806, when Benjamin Smith Barton wrote to Cuvier (1834, Vol. II, p. 270, and elsewhere) "qu'un voyageur intelligent a vu dans un endroit particulier, pres de la rivière des *Indiens Osages,* des milliers d' ossemens de cet animal [the American mastodon], et qu'il y a recueilli, entre autres, dix-sept défenses, dont quelques-unes avait six pieds de long et un pied de diamètre: mais la plupart de ces os étaient dans un grand état de décomposition." Barton sent a molar to Cuvier, who placed it in the Cabinet du Roi.

Probably at this time and certainly within the following thirty years, the American Philosophical Society received various specimens, mostly of mastodons, from Benton County. Some of Hays's types of species now considered synonyms of *Mammut americanum,* described by him in 1834, are supposed to have come from there, although there appears to be some confusion of record between this region and Big Bone Lick. The Society's interest in the Benton County finds culminated in 1844, shortly after the period here considered, in the purchase of the large collection of S. H. Whipple. Such of these specimens as survive and are still recognizable as to origin are

now to be found in the Academy of Natural Sciences of Philadelphia.

The most famous, or notorious, collection from Benton County was, however, that made in 1840 by Albert Koch, which included his "Missourium" skeleton along with bison, deer, megathere, and mylodont bones. Koch's activities are mentioned elsewhere in this paper and are now cited only as including another relatively early western discovery.

Although the Pleistocene mammals of western Missouri might be considered as representing the extension of the eastern fields, rather than as discoveries in what we now think of as truly western fossil fields, no such qualification can apply to the discovery of bones of the same geological age in Oregon in December, 1839. These remains were found by a Mr. Ewing Young on the "Walhammet" (Willamette) or Multnomah river, a tributary of the Columbia, in latitude 44° N. (hence near the present city of Eugene, Ore.). They came into the hands of Mr. H. C. Perkins of Newburyport, Mass., who described them in a letter published in the *American Journal of Science* (Perkins, 1842). Although he proclaims himself "a tyro in palaeontology" (thus reflecting a general recognition of that science as such and of the technicalities already involved in proficiency in it), Perkins' descriptions and figures are recognizable today and appear to be accurate.[47] The specimens included parts of "fossil elephant" (mammoth), a tarsal of the "fossil ox" (extinct bison), a tooth correctly referred to *Mylodon* Owen, and a humerus also correctly referred to "the megatherioid tribe," *i.e.*, in later language, to the Gravigrada or ground-sloths. (In fact, the humerus, like the tooth, belonged to *Mylodon*; these parts had not then been found in unequivocal association.)

The early discovery of fossil vertebrates in Alaska may be mentioned, as being literally in western North America, although from the historical point of view as regards exploration in this period Alaska might perhaps better be considered as part of Siberia.

Mammoth remains were found in Alaska on August 8, 1816, by the exploring party under Kotzebue (see Kotzebue, 1821; Maddren, 1905, has an excellent account of these occurrences and cites and quotes various early authors, not all of whom are mentioned in the present condensed account). Dr. Eschscholtz, whose name was and still is attached to the bay in which the discovery was made, found that supposed mountains along the shore were in fact pure ice, with the surface covered with dirt. Kotzebue wrote, "An indisputable proof that what we saw was real ice, is the quantity of mammoths' teeth and bones, which were exposed to view by the melting, and among which I myself found a very fine tooth." In 1827 an English expedition under F. W. Beechey visited the locality, which they named Elephant Point, and collected fossils that were described by Buckland (1831). His faunal list included "Elephant" = Siberian mammoth, "Urus" = extinct bison, musk-ox, "Deer" = caribou, and horse. The musk-ox and some of the caribou remains were considered modern, but these animals did exist in the Pleistocene of the region and the specimens of them were quite possibly fossils. Many other expeditions have since visited the locality, which continues to yield specimens in considerable numbers.

Finally it may be mentioned that Mexico, where Europeans first saw fossil bones in the New World, had by 1842 yielded more specimens in some variety, including mastodons and horses (see Meyer, 1840, Cuvier, 1834 and elsewhere). (It is remarkable that after more than four centuries of continual, if usually casual, discovery and sporadic study, the undoubtedly abundant fossil vertebrates of Mexico are still very incompletely known and have received little really systematic attention.)

Scientific Periodicals

"It is painful to perceive what conspicuous blanks are yet left for America to fill up, and especially in those important branches, American geology and American organic remains. This feeling is greatly increased by the occasional taunts and sneers we see directed against us, in foreign scientific works. . . . It is for this object, as well as for the diffusion of the love of science at home, that this monthly journal is about to be established."—Prospectus of Featherstonhaugh's *Monthly American Journal of Geology and Natural Science*, 1831.

The rise of a science involves not only expanding knowledge and changing ideas but also more physical requirements: trained men, materials, libraries, laboratories, instruments, and means of recording and spreading information. During the

[47] The derogatory summary by Hay (1902) of this pioneer paper on western fossils could not have been written by any fair critic who had read the paper, which is, in fact, excellent for its period.

period under consideration, American vertebrate paleontology required and acquired no special facilities in the way of laboratories and equipment. Its libraries were those of the early learned societies, an exceptionally important historical subject but one not particularly related to this one facet of scientific history. Something of the development of professionally competent students and of the acquisition of specimens has been related. It remains to mention briefly the rise of publication facilities with respect to vertebrate paleontology.

Until well into the nineteenth century a principal means of diffusing scientific information was not publication, as we know it, but personal correspondence. Such correspondence was not an informal affair, lightly entered into, but a serious responsibility carefully arranged and religiously maintained. The seventeenth and eighteenth century American naturalists acquired and contributed knowledge almost entirely through their correspondents, usually European pundits. Even in the nineteenth century one finds no less a man than Benjamin Smith Barton contributing to vertebrate paleontology not by any publications of his own but by gathering "intelligence" and transmitting it to his paleontological correspondents, among whom was Cuvier. In 1811 Jefferson was asked by Count John Potocki to enter into a treaty of correspondence on this and related subjects and, pleading old age,[48] he transferred the petition to Barton. It often appeared that the early American scientific journals were less mediums for papers prepared as such for the press than printed compilations of the editors' scientific correspondence.

In frank imitation of such venerable institutions as the Royal Society of London, formal memoirs were read before the American Philosophical Society and the American Academy of Arts and Sciences. No little information has been lost because many of these papers were left in manuscript, but in 1769 the Philosophical Society began to publish *Transactions* and in 1785 the American Academy followed suit. The latter publication has been of comparatively minor importance for vertebrate paleontology, despite eminence in other fields, but the *Transactions* of the American Philosophical Society, oldest of American scientific periodicals, published the relatively large number of twelve papers in this field between 1799 and 1842, while seven more appeared in the *Proceedings*, printing of which was begun in 1838. During their shorter span prior to 1843, ten papers on this subject appeared in the *Journal* (1823–1842) and *Proceedings* (1841–1842) of the Academy of Natural Sciences of Philadelphia. Most of these were also by members of the Philosophical Society and the two represent essentially the same research center. The publications of the Society continued to be and are today among the most important mediums of publication for this science. Although very active in the epoch of Leidy and Cope, the Academy thereafter almost ceased to work or publish on vertebrate paleontology, but there has been some revival of interest in the last year or two.

The other important society publication for early vertebrate paleontology was the *Annals of the Lyceum of Natural History of New York*, publication of which began in 1823 and which had ten papers on this subject in 1824–1842. This periodical also survives, as the *Annals of the New York Academy of Science*. Since 1842 it has published a few very important vertebrate paleontological contributions, but such publication has been quite sporadic.

The first scientific journal in the United States, as opposed to society transactions and the like, was probably Mitchill's *Medical Repository*, New York, founded in 1797. Many such ventures were begun early in the nineteenth century, in almost all cases as personal undertakings of scientific men with wide correspondence. As a few examples there may be mentioned Barton's *Philadelphia Medical and Physical Journal*, 1804, Coxe's *Emporium of Arts and Sciences*, 1812, Featherstonhaugh's *Monthly American Journal of Geology and Natural Science*, 1831, and Rafinesque's *Atlantic Journal and Friend of Knowledge*, 1832. All of these were published in Philadelphia and all were short-lived, as was Mitchill's journal. They included some notices of fossil bones, but these involved little in the way of original contributions except for Featherstonhaugh's journal, which had an important paper on "Big Bone Lick" by Cooper.

As regards our subject, the greatest of all journals is unquestionably the *American Journal of Science*, founded by Benjamin Silliman of Yale University in 1818. Its first issue contained instructions for collecting fossils. In 1820–1842 the astonishing number of 43 papers or notices on fossil vertebrates appeared in that periodical and it has ever since continued and is today a leading

[48] It turned out that Jefferson outlived Barton by eleven years.

medium in this science. Perhaps no other periodical in the world has published so many contributions to vertebrate paleontology (see Lull, 1918).

The State of Knowledge in 1842

"Is not science a growth? Has not science, too, its embryology? And must not the neglect of its embryology lead to a misunderstanding of the principles of its evolution and of its existing organization?"—Spencer.

So much has been accomplished by American vertebrate paleontologists in the last century that we fall into the habit of thinking that everything worth while in our field is the product of this period. We still think of ours as a young science, and in doing this we make the mistake of those beauties who so acquire the habit of being thought young in their twenties that they continue to think that they are young in their sixties. As sciences of comparable scope go—excluding such broad categories as biology or chemistry—vertebrate paleontology is now an old science. It is, for instance, considerably older than nuclear physics or genetics or psychology. Unlike these sciences, it was a going concern a century ago, with a defined field of study, a group of recognized specialists, a body of organized materials, a sound methodological routine, and a systematized set of theories.

By way of review and summary, the significance of the development followed on the preceding pages may be judged by statement of the extent of its results in 1842.

Vertebrate paleontology was then in what is usually called the Cuvierian stage, because Cuvier gathered together the products of many decades of work by many students, weeded out most of the errors and contradictions, and left a generally sound and consistent system. Fossil bones were known to be the remains of animals. These animals were known to be entirely extinct, with a few exceptions among the most recent of them. They were known to have lived through a long sequence of epochs, extending over millions of years. It was known that each successive epoch had characteristic and different animals, that fish appeared before reptiles and reptiles before mammals and that within each major group the species were relatively shortlived and were continually replaced by other species. It was known that species differed from each other in more or less constant morphological characters of bones and teeth and that they could be defined by determination of these points of difference. It was felt that species were nevertheless related and that their closer or more distant affinities could be determined by greater or lesser resemblances in their fossilized remains. It was known that structure and function were correlated and that habits could to some extent be determined from morphology.

American fossils and American students had contributed to these results, which were nevertheless more largely dependent on work done in Europe. American paleontology had been for a time more or less parasitic on European and was still satellitic, but it was rapidly ceasing to be either. The principles developed mainly in Europe were now as well known and as well used in America as anywhere. Americans had achieved professional status and had laid a solid basis on which Leidy and his successors were to stand, fully co-operative in international science but in no sense inferior. This achievement had been encouraged by the American scientific societies and would have been long delayed or impossible without them. The American Philosophical Society had been the most important single factor of this sort.

In this broader field of principles and theories, the most important things that were not known were the nature of the relationships between species and the mode of origin of the new species that continually appear in the sequence. The hypotheses of affinity by common ancestry and of origin by evolution had been stated and were being discussed, in America as well as in Europe, but they were not generally accepted and, indeed, the evidence did not strongly support them. It was vaguely felt that species appeared by creation or spontaneous generation—which were merely pious ways of repeating the obvious fact that no one knew how they appeared. Their affinities might mean that they were closer or more distant copies of some abstract and supernal model—which was not much more than a sly philosophical way of ignoring the observed relationships and begging the question. In these respects the last century has seen real and fundamental progress, far as we still are from the ultimate goal.

The system of taxonomy that we use now was already definitely established and in use. The details of classification and nomenclature have changed greatly with the complication of increase in number of known groups and with clarification

and standardization of concepts, but the basic method has not changed in the last century. Americans had contributed nothing to the invention of this method, but by 1842 they had adopted it and they were already destined to be among the most important students to develop and to perfect it.

On the more concrete plane of the actual quantity of knowledge, the number of known facts rather than the methods for their study and arrangement and the theories for their interpretation, the difference between 1842 and 1942 is much greater. This is an inevitable result of the sometimes misunderstood phenomenon that the rate of accumulation of facts depends very largely on the existing means for using those facts in some scientific way once they have been acquired. Nevertheless, the student who feels that American vertebrate paleontology began with Leidy may be astonished at the extent of knowledge before Leidy identified his first fossil.

In 1842, Paleozoic vertebrates were just becoming known in America. The Devonian fish *Sauripteris* was found in 1840, but published notice of it was not to appear until 1843. In 1841 Sir William Logan had found Carboniferous footprints in Nova Scotia, and he soon briefly mentioned them (Logan, 1842), although serious study of them was long delayed.[49] Despite this understandable lag as regards the Paleozoic, the eastern Triassic faunas were already rather well known in 1842. The Connecticut valley tracks were known in considerable variety and it was correctly believed that some of them were made by extinct reptiles. A few bones of these reptiles had been found but could not yet be clearly classified. Triassic fishes from Massachusetts, Connecticut, New Jersey, and Virginia had been well classified.

Jurassic deposits are practically absent from the parts of North America that had then been explored and no vertebrates of that age were known, but a considerable fauna had been found in the Cretaceous from the Atlantic coast to South Dakota and Montana. This included a variety of fishes and reptiles—dinosaurs among them although the entity "dinosaurs" had not been defined. The western Cretaceous mosasaurs were already represented by good specimens. Included among the supposed Cretaceous fishes and reptiles there were probably some that are really Eocene—a confusion that is even now not entirely cleared up, so that the students of 1842 cannot be very severely condemned for it.

The great faunas of American Tertiary land vertebrates were still quite unknown, although the discovery was to follow almost immediately. A beginning had been made on Tertiary marine vertebrates. *Basilosaurus*, the Eocene cetacean, was discovered in 1832 and after some understandable fumbling its general structure and affinities had become reasonably well known by 1842. The first Miocene dolphin was described in 1842. Tertiary shark teeth, mostly Miocene in age, had been gathered as curiosities since pre-Columbian times and had received some able, technical study by 1842.

FIG. 23. Sites and dates of some of the fossil vertebrate discoveries in eastern United States prior to 1843. The dates are in general those of discovery but in some cases are dates of publication, see the text. These are only a few of the outstanding discoveries. In 1842 Pleistocene mammals, particularly, were also known from dozens of other localities scattered all over the region covered by the map.

[49] See Lambe, 1904, who also summarizes the later history of Canadian vertebrate paleontology. Although the true discovery of American fossil vertebrates was made by a Canadian in what he claimed as part of Canada and although Canadian invertebrate fossils were described in the eighteenth century and continually since then, almost nothing of value was done with strictly Canadian fossil vertebrates until about 1860. Since that time there have been very eminent vertebrate paleontologists in that part of America—Dawson, Whiteaves, G. F. Matthew, Lambe, and Parks, to mention only a few—but Canada hardly figures in a history of the science limited, like this, to the years before 1843.

The Pleistocene mammalian fauna, which includes strikingly large animals, which is often well-exposed in unconsolidated deposits, and which may occur almost everywhere in eastern North America, was of course much the best known part of vertebrate paleontology. The rich microfauna had not yet attracted attention, but almost all the main types of larger extinct Pleistocene mammals were already familiar. The only extinct rodent surely distinguished was the giant *Castoroides*. Among carnivores, bears and the great cat *Panthera atrox* had been found as fossils, but it was not yet established that these were extinct species. Bones of the living species of walrus were known from as far south as Virginia. Edentates had received special attention. The material was still incomplete, but all three of the eastern types of ground sloths, *Megalonyx, Mylodon,* and *Megatherium,* had been found and distinguished and their affinities were as well known as was possible before discovery of the annectant and ancestral older South American types. Cetaceans of probable Pleistocene age were known, but had attracted little attention because they did not appear to be distinct from living forms—still true of the great majority of Pleistocene cetaceans.

Much more work had been done on proboscideans than on any other fossil vertebrates. The American mastodon, now called *Mammut americanum,* was completely known and well understood. Competently restored mounted skeletons had been on exhibition for some forty years. There was still some disagreement as to whether the great amount of observed variation was intraspecific or represented generic and specific diversity, but the more able students held the former (correct) view. One other valid species, *Stegomastodon chapmani,* had been found and for all practical purposes lost again. Mammoths had been distinguished from mastodons and it had been suggested, but not clearly established, that some American mammoths were distinct from those of Siberia and Alaska, where these fossils were also known. Mastodon remains had been found at localities far too numerous to mention over a great area from New England to western Missouri.

The occurrence of sirenian bones along the Atlantic coast from New Jersey to Georgia had been observed, but identification had gone no further than to establish their generally manatee-like character.

True fossil horses were known from scattered localities in New Jersey, Pennsylvania, Maryland, Kentucky, Louisiana, etc., but they had not been clearly distinguished from the closely similar living species. Harlan had claimed discovery of a fossil tapir, but the identification was queried or rejected—although fossil tapirs do occur under like circumstances, it still is not certain that Harlan really had one.

A fossil peccary had been found and correctly identified as to family, but pertinence to an extinct genus and species had not been clearly established and the discovery was not widely known even among paleontologists. The rare and peculiar cervid now called *Cervalces scotti* had been found and properly distinguished. Other fossil deer belonging or very nearly allied to recent species had also been discovered. The great-horned *Bison latifrons* was known and named. Small-horned Pleistocene bisons had also been found in Kentucky, Oregon, Alaska, and probably elsewhere, but it had not been proved that they belonged to extinct species. The remarkable extinct muskoxen now called *Boötherium bombifrons* and *Symbos cavifrons* were both known, although the distinctions between them and between the latter and the living muskox were not completely clarified. The recent species of muskox had also been found fossil in Alaska.

Among North American Pleistocene mammals of moderate to large size, something was known of almost every well-marked group with the probable exceptions of the glyptodonts, large canids, and camels. In judging the importance of these few major omissions, it must be remembered that these are relatively rare fossils in regions then belonging to the United States, aside from Florida which was a recent and little-studied acquisition.

Looking backward from the vantage point of 1942, the knowledge available in 1842 seems small but highly significant—and its greatest significance is that without it we would not have nearly so high a vantage point now. If one could have looked forward from 1742, the progress that was to be made in the ensuing century would have appeared incredibly great.

Appendix

A. Records of Early Vertebrate Paleontological Activities of the American Philosophical Society

In 1838 the Society started publication of its *Proceedings* and thereafter its more important paleontological activities were made generally

available at the time and are cited as contemporaneous publications in various bibliographies. Before that date a large amount of work concerning vertebrate paleontology was done, but only a small proportion of this was published in the *Transactions*. The surviving records of these earlier activities, to and including 1837, are very incomplete but are of great historical interest. They consist of manuscript proceedings, printed in 1884, and letters to the Society or its officers, most of which have never been printed but are preserved in the Archives in the Society's Library. Notes on these are given below in temporal sequence. The notation "(Proc.)" refers to "Early Proceedings" (Amer. Philos. Soc., 1884), and the notation "(Ms.)" refers to letters in the Archives. (The list of letters in the Archives has been supplied by Mrs. Gertrude Hess.)

Mar. 5, 1784. (Proc.) "Nicola presented Observations on petrified bones found near the Ohio; thigh-bone, tusk and grinder, brought to the city by Maj. Craig." [Mr., later General, Lewis Nicola was elected Curator of the Society in 1769 and again in 1779–1785, inclusive. He does not appear to have published any observations on fossils. This is the first recorded connection of the Society, as such, with fossil vertebrates, although antedated by personal contacts of Franklin, Jefferson, and other members.]

Oct. 5, 1787. (Proc.) "A large thigh bone found near Woodbury creek in Glocester [*sic*] county, N. J. [perhaps a Hadrosaurus], was described in a paper by Mr. Matlack and Dr. Wistar; who, with Dr. Rodgers, were requested to search for the missing part of the skeleton." [If the secretary's conjecture bracketed in the printed version is correct, as it may be, this was the first dinosaur found in North America, or probably in the world, but verification is now impossible and this cannot be accounted a true discovery. The paper was never printed, and there is no record of the results of further search. Mr. Matlack was Timothy Matlack, an early member, secretary of the Society in 1781–1783. He died in 1829. He does not appear to have published any of his paleontological observations.]

Mar. 18, 1791. (Proc.) "T. Matlack's description of 'a large Tusk found in the back county' was read, and two pieces of the tusk exhibited." [See preceding note.]

Mar. 10, 1797. (Proc.) Resolution on Jefferson's memoir on *Megalonyx* [published in 1799], referring it to the publication committee, instructing them to have illustrations prepared, and requesting Peale to put the bones "in the best order, for the Society's use."

May 26, 1797. (Proc.) A letter from J. Caffery of Nashville tells of mammoth bones at Manscoes Lick near the mouth of the Cumberland and offers to make a collection which was authorized to the extent of $100. [The outcome of this plan is not recorded.]

July 21, 1797. (Proc.) Reading of Turner's memoir on mammoth bones and its reference to James, Wistar, and Seybert. [The memoir was published, 1799, and has been discussed above. Thomas C. James and Adam Seybert were secretaries of the Society, not personally concerned with fossil bones.]

Jan. 19, 1798. (Proc.) "Jefferson presented a Bone of the Mammoth some time ago found in Virginia."

Oct. 10, 1798. (Ms.) Samuel Hodgdon, Philadelphia, to Jonathan Williams. Three barrels of bones received from Pittsburg [possibly from Big Bone Lick].

Mar. 1, 1799. (Proc.) "Wistar's description of the Bones of the unknown animal, referred to Dr. Shippen." [The *Megalonyx* bones, previously described by Jefferson. The entry shows that Wistar's work was completed in 1799, not 1797. Several Shippens were early members. This one was probably Dr. William Shippen, Sr., a former vice-president.]

Mar. 15, 1799. (Proc.) "Dr. Shippen reported Dr. Wistar's descriptions of the Bones to be very accurate. Publication ordered." [Published the same year.]

July 24, 1801. (Ms.) Charles Willson Peale, Philadelphia, to Robert Patterson. Requests loan of $500 from the Society to help complete the skeleton of the mammoth.

July 24, 1801. (Proc.) Special meeting called to consider Peale's request for a loan. Resolved to make the loan for four months, without interest.

Aug. 21, 1801. (Proc.) Peale's bond and mortgage received and $500 paid to him. [This enabled completion of his excavations, as discussed on a previous page.]

May 7, 1802. (Proc.) Peale is allowed to leave part of his museum in the Society's Hall while the remainder is moved to the State House [Independence Hall]. [For sequels, see Jan. 17, 1806, and Dec. 6, 1811.]

June 10, 1802. (Ms.) Samuel Brown, Lexington, Ky., to John Vaughan. Discusses a skull

"of the *Ox Kind*" forwarded to the Society and argues against the possibility that "the two kinds of large teeth" belonged to the same animal at different ages. [The ox-like skull was that later named *Bison latifrons* and first described by Rembrandt Peale, 1803a. Contrary to occasional statements, this historic type was not from Big Bone Lick, proper, but from a site ten or fifteen miles distant. Unfortunately this letter from Brown gives no details of the discovery. The two kinds of large teeth were doubtless those of the mammoth (now so-called) and the mastodon. Although there is little evidence that he, personally, collected or studied fossil bones, Dr. Brown deserves notice in paleontological history because he was instrumental in obtaining some notable specimens for the Society. See also Nov. 1, 1805, and immediately following entries; also Oct. 17, 1806.]

Mar. 7, 1804. (Ms.) W. Lewis, Campbell Co., Va., to Thomas Jefferson. Donates a bone and some rocks to the Society.

Sept. 17, 1804. (Ms.) Tristram Patton, Second Creek, Monroe Co., Pa., to William Hembel, Jr. Respecting fossil bones intended for a museum.

April 9, 1805. (Ms.) Samuel Brown, Lexington, Ky., to John Vaughan. Brown will forward bones to the Society. [See below.]

April 27, 1805. (Ms.) William Bartram, Kingsessing, to ————. Bones to be forwarded to [Jefferson at] Monticello.

Nov. 1, 1805. (Proc.) Donation "from Dr. Sam Brown of Lexington, Kentucky, Cranium, tooth and pieces of bone found in Saltpetre cave."

Nov. 4, 1805. (Ms.) Samuel Brown, Lexington, Ky., to Thomas Jefferson. Skull and jaw of unknown animal sent to the Society.

Dec. 20, 1805. (Proc.) "From Dr. Brown of Kentucky, the head bones of a new animal found in Saltpetre cave. . . . The cave bones referred to Dr. Wistar." [See next entry.]

Jan. 17, 1806. (Proc.) The skeleton of a recent Asiatic elephant is to be placed in the same room with Mr. Peale's "mammoth" [mastodon]. [Thus the mastodon skeleton was still in the Society's Hall although the Peale Museum in Independence Hall had been established and the mastodon was later moved there, see May 7, 1802, and Dec. 6, 1811.]

"Dr. Wistar reported that the Cave bones 'prove to be the Bones of the Head of the Pecary of S. America, as described by Daubenton in Buffon's works.'" [These are the bones concerned in previous correspondence with Dr. Brown, who presented them to the Society. Brown described the cave, in Rockcastle Co., Ky., in *Trans. Amer. Philos. Soc.*, VI, 1804, pp. 235–247. Wistar's identification is correct as far as it goes, and is much the oldest identification of an American fossil peccary, although overlooked because not published at the time. Leidy described this fine specimen and named it *Euchoerus macrops*, 1853. It is now believed to belong to *Platygonus compressus* and is still preserved in the Academy.]

June 20, 1806. (Proc.) Wistar is to communicate with Jefferson regarding bones supposed to be in New Orleans. [No particulars are given. Reference is probably to the Goforth collection, which was taken to New Orleans by Ash at about this time.]

Sept. 19, 1806. (Proc.) "Dr. Wistar reported, that Jefferson would attend to the bones found in Ohio." [Probably the same collection as that concerned in the preceding entry.]

Oct. 3, 1806. (Proc.) "Tooth of the Mammoth from Montgomery county, Virginia, exhibited by Dr. Barton." [Details of this discovery seem to have been lost. It is not cited in later compilations.]

Oct. 17, 1806. (Proc.) Brown's and Jefferson's letters about mammoth [mastodon] bones referred to Wistar, Peale, and Vaughan, who are to correspond with Dr. Goforth. [Wistar wrote to Goforth on Dec. 1, 1806. The letter was published in *The Navigator*, 1814, p. 201, and is copied in Jillson, 1936, p. 39. "Brown" is Dr. Samuel Brown of Lexington. "Vaughan" is John Vaughan, frequently mentioned in the records because he cared for much of the Society's correspondence, although he apparently had no other personal interest in fossil vertebrates.]

Nov. 21, 1806. (Proc.) Part of a mammoth [mastodon?] jaw is donated. [No particulars.]

Jan. 23, 1807. (Ms.) William Goforth, Cincinnati, Ohio, to Thomas Jefferson. Details concerning mammoth [mastodon] bones. [This is the letter given without date in *The Navigator*, 1814, and Jillson, 1936. It has been discussed on a previous page of this paper.]

Feb. 25, 1807. (Ms.) Thomas Jefferson, Washington,D. C., to Caspar Wistar. Encloses letter from Goforth and proposes to obtain a collection for the Society from Big Bone Lick. [This famous letter has been printed in Jefferson's collected writings and frequently copied, *e.g.*, Os-

born, 1935, Jillson, 1936. The enclosure was the letter of the preceding entry.]

Mar. 6, 1807. (Proc.) Jefferson proposes to obtain mammoth [mastodon] bones for the Society. A list of those wanted has been sent by Wistar. Dr. Goforth's letter to Jefferson is received. [As noted on a previous page, the upshot of these negotiations was that Clark, not Goforth, was employed to make a collection at Big Bone Lick and that a large number of specimens were duly received by Jefferson and by the Society.]

Feb. 3, 1809. (Proc.) Wistar read a paper on bones procured by Jefferson from Big Bone Lick. Referred to Barton, Peale, and McDowell. [The Clark Collection. Wistar's manuscript was lost, see next entry. J. McDowell was Councillor of the Society.]

Mar. 17, 1809. (Proc.) Wistar's manuscript has been lost and an advertisement for its return is to be inserted in a public paper. [The manuscript was not recovered.]

June 21, 1811. (Proc.) Dr. [Benjamin Smith] Barton exhibited a *Megatherium* claw from a cave at the foot of Cumberland mountain in Tennessee. [*Megatherium* is not reported from Tennessee in recent compilations. This may have been *Megalonyx* or *Mylodon*, but the specimen is not now known.]

Dec. 6, 1811. (Proc.) Peale's Museum has been entirely removed [from the Society's Hall] to the State House [Independence Hall]. Peale's request to cancel his lease referred to Patterson, Vaughan, and Collins. [In March, 1812, the rooms were leased to Mr. Sully and Peale's lease terminated.]

Dec. 2, 1817. (Proc.) George Chambers, Chambersburg, Pa., to John Vaughan. Forwards bones requested by the Society. [Proboscidean remains had been found near Chambersburg at least as early as 1806. This doubtless refers to a later find of the same sort.]

Dec. 20, 1817. (Proc.) G. G. Bogert, New York, to John Vaughan. Encloses documents relative to fossil "elephants."

Aug. 20, 1824. (Proc.) Dr. DeKay [of the New York Lyceum] to be permitted casts of the Society's *Megalonyx* bones.

Jan. 21, 1825. (Proc.) Le Sueur permitted to draw such bones as have already been described in the *Transactions*.

April 1, 1825. (Proc.) Harlan read a paper on a fossil tapir, a hare, and a new genus of rodents, *Osteopera*. Ordered "enrolled in the archives." [These observations were included in Harlan's *Fauna Americana*, 1825. *"Osteopera"* is still a puzzle. It was apparently an aguti, but no other fossil aguti has ever been found in the United States. On the other hand, the explanation that it was a recent, imported specimen is not entirely probable.]

June 15, 1825. (Ms.) Jer. Van Rensselaer, New York, to John Vaughan. The New York Lyceum forwards a cast of the jaw of *Megatherium* to the Society. [This was undoubtedly the jaw from Skidaway Island, Georgia, the first surely identified *Megatherium* from North America.]

Dec. 2, 1825. (Proc.) Dr. [Samuel] Colhoun presents a drawing of a mammoth to the Society.

Jan. 31, 1829. (Ms.) William Cooper, Philadelphia, to John Vaughan. Mr. Doifeville of Cincinnati has *Megalonyx* bones and it is hoped that the Society may acquire them. [I find no evidence that this was done and no subsequent reference to these bones.]

May 20, 1831. (Proc.) Hays read a paper on mastodons. Referred to Lea, Pickering, Wetherill.

June 17, 1831. (Proc.) Hays' paper reported for publication. [Published 1834.]

Sept. 16, 1831. (Proc.) Casts of mammoth bones to be given to the Academy of Natural Sciences.

Oct. 29, 1831. (Ms.) Richard C. Taylor of the Athenaeum, Philadelphia, to John Vaughan. Harlan is to have access to fossils in the Society's rooms. [The Athenaeum then occupied part of the Hall.]

Dec. 2, 1831. (Proc.) Dr. Hays invited to exhibit mastodon bones borrowed from various museums. A paper by him referred to committee.

Dec. 16, 1831. (Proc.) The bones exhibited by Hays with remarks by Harlan. Hays' paper to be published.

July 20, 1832. (Proc.) A donation of bones from Mr. Bry of Arkansas referred to Harlan, Pickering, Horner. [See next entry.]

Sept. 21, 1832. (Proc.) Committee on Bry's bones reported by letter. Bones referred to Harlan for description. [These were the first known remains of *Basilosaurus* or *"Zeuglodon."*]

Mar. 15, 1833. (Proc.) Moulds of mastodon bones have been made. Six sets of casts ordered.

Oct. 4, 1833. (Proc.) Sets of these casts to be sent to Paris and London. Curators to report on distribution of casts.

April 18, 1834. (Proc.) Curators report that mastodon casts cost $30 and are ready for exchange or sale at that price.

B. *The Collection of Vertebrate Fossils of the American Philosophical Society*

Thanks to the efforts of Jefferson and of numerous others before and after him, the Society had in the early nineteenth century a collection of American vertebrate fossils that was probably the largest and most nearly complete of any then in existence. When its cabinet was started, the Society was the only institution in America where permanent and satisfactory care for scientific specimens was provided. By about 1842, however, there were numerous other institutions for the storage, study, and exhibition of such specimens and it became increasingly evident that the rather embarrassing function of curatorship of so bulky a collection was not necessary to the Society and that it interfered with the primary aims of a national organization for the promotion and diffusion of useful knowledge. In Philadelphia the Academy of Natural Sciences had taken over the more local and specialized functions of a museum and research institution, and the collections of the Society were deposited in the Academy toward the end of 1849.

Approximately 160 years have elapsed since the Society began its collection of fossil vertebrates and in that period the vicissitudes of time and the vagaries of individuals have caused the loss of many specimens. Others, although they still survive, have become dissociated from records of their origin and can only be shown by circumstantial evidence ever to have belonged to the Society. Doubtless many in this category have been overlooked and it is likely that this was the source of numerous specimens in the Academy that are not labeled as coming from the Society. The following is a summary list of specimens now in the Academy and known to have come from the Society's collection, prepared by me from the Academy's catalogues and other records, made available through the kindness of Dr. Edwin Colbert and Mr. Robert Chaffee. The systematic names are as far as possible those now in use, although actual reidentification of the specimens has not been attempted and doubtless would result in changes. In the light of the dates, the list is impressive and it should be emphasized that it includes only a portion, probably less than half, of the materials that were at some time in the Society's collections.

Fishes:

Carcharodon megalodon. 2 teeth. Ricehope Estate, Cooper River, South Carolina. Presented by Thomas Jefferson. The Society is known to have had other shark teeth, but these were not recognized in the Academy's present collection.

Saurocephalus lanciformis. Jaw fragments. Type. Missouri River. Collected by Lewis and Clark.

Unidentified. Vertebra. Paris.

Reptiles:

The Society is known to have had a number of fossil reptiles, mainly from the late Mesozoic and early Tertiary of New Jersey, but these have not been identified in the Academy's records.

Mammals:

Carnivores:

Ursus americanus. Skull. A cave in Kentucky.

Panthera [or *"Felis"*] *atrox.* Jaw. Type. Natchez, Miss. Collection of William Henry Huntington, presented to the American Philosophical Society in 1836. Although not so recorded, it is probable that the Society's specimens of *Equus complicatus* from Natchez were also part of the Huntington collection, which is also believed to have included mastodon specimens, not identifiable in the Academy's records.

Edentates:

Megalonyx jeffersonii. Foot and limb bones. Type. West Virginia.

Cetaceans:

Basilosaurus cetoides. 4 vertebrae. "Alabama." "Dr. S. G. Morton." Not catalogued as from the Society. There is evidently much confusion, but this lot, catalogued under numbers 12944 and 12949, either is or includes the first specimens of *Basilosaurus,* sent by Judge H. Bry to the Society in 1832 and from the Ouachita or Washita River in Louisiana (not Alabama). Although this material was the author's hypodigm for the genus, *Basilosaurus,* it does not include the technical types of the species *B. cetoides.*

Proboscideans:

Since the Society is authoritatively said (by Wistar and others) to have had almost every bone of the mastodon skeleton by 1808, at latest, it is

obvious that the greater part of this collection was lost or is not now recorded as having belonged to the Society.

Mammut americanum. 3 mandibles, 19 molars, and 1 patella from Big Bone Lick, at least in part from the Clark Collection made for Jefferson, including Hays' types of *"Mastodon jeffersonii," "M. cuvieri,"* and *"M. godmani."* 1 skull, "Ohio," perhaps from Big Bone Lick, Kentucky. 1 jaw without locality data, type of *"M. collinsii"* of Hays. 10 tusks, 19 mandibles, 2 upper tooth series, 83 molars, and 27 vertebrae from Benton County, Missouri. 3 molars from Pittston, Luzerne Co., Pennsylvania. (Most of the material from Benton Co. was not acquired until 1844.)

Stegomastodon chapmani. Molar. Type. Locality unknown.

Mammonteus primigenius. 11 molars from Big Bone Lick (probably Clark Collection). 7 molars and 1 mandible from Benton Co., Mo. 4 molars without locality date. Most of these specimens, perhaps all, will probably prove to belong to other species of mammoths when restudied.

Stegodon insignis. Two molar fragments and a cast of the cranium. Siwalik Hills, India. From Rev. James R. Campbell.

Stegodon elephantoides. Part of a molar. Siwalik Hills, India. Rev. James R. Campbell.

Unidentified proboscideans. A tusk fragment and part of a rib from the Siwalik Hills, the former (probably also the latter, without record) from Rev. James R. Campbell.

Perissodactyls:

Equus complicatus. Molar. Paratype. Natchez, Miss. Two molars. Pittston, Luzerne Co., Pa. Four bones. Big Bone Lick.

Equus sp. Partial mandible. Natchez, Miss.

Artiodactyls:

Platygonus compressus. Skull and jaws. Type of *Euchoerus macrops*. Saltpetre Cave, Kentucky. From Dr. Samuel Brown.

Hippopotamus amphibius. Molar. Val d'Arno, Tuscany.

Hippopotamus sivalensis. Molar. Siwalik Hills, India.

Cervalces scotti. Skull—Type—and two antler bases and four other bones. Big Bone Lick. The type is surely and the others are probably from the Clark-Jefferson collection.

Boötherium bombifrons. Skull. Big Bone Lick. Clark-Jefferson collection.

Symbos cavifrons. 5 molars. Natchez, Miss. 1 metacarpal. Big Bone Lick. Clark-Jefferson collection.

Bison antiquus. Horn core. Type. Big Bone Lick.

Bison latifrons. Horn core and base of skull. Type. "Big Bone Lick"—really about ten miles from the lick, itself. (Presented by Dr. Samuel Brown, but not so recorded.) One molar. Susquehanna River near Pittston, Luzerne Co., Pa.

Bison sp. Two vertebrae. Big Bone Lick.

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All of the publications here cited (and a great many more) have been consulted either by or for the author in preparing this paper. This is in no sense an adequate bibliography of the subject but it includes most of the important literature, whether or not it is explicitly cited in the text. Where there is a choice of editions or sources, accessibility has been considered. For instance most biographical notices are cited from the concise and widely available *Dictionary of American Biography*, which contains references to other more detailed or older sources some of which were consulted without being listed here. Similarly Goode's important historical studies are cited in the collected edition of 1901 although they appeared separately at various earlier dates. When two dates are given, that in parentheses is the date of the reading of a paper or of its first more obscure publication and that without parentheses is the date of an available printed version. The best general bibliography is in Hay, 1902, although it is very incomplete for this early period. Jillson, 1936, has an excellent bibliography of Big Bone Lick, most important single early fossil locality, and Osborn, 1936, has a nearly exhaustive bibliography of literature on the mastodon, the most important early fossil discovery.

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* According to the records of the Am. Philos. Soc. the paper listed by Hay, 1902, as Hays, 1842B, was issued in 1843.

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† The name was habitually written "Clarke" by his friend Jefferson and many others and it so appears more often than not in contemporary literature including early editions of the journals. It is "Clark" on the title of this edition, but "Clarke" in the text.

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THE AMERICAN INDIAN AND THE AMERICAN PHILOSOPHICAL SOCIETY

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(Read February 14, 1942, in Symposium on the Early History of Science and Learning in America)

THE American Indian presented a challenge to scholars in Europe and the American colonies. The Indians possessed no history, no satisfactory traditions of origin, they spoke some 1500 languages, were composed of about as many autonomous social units, presented many anatomical variations and great differences in their ways of life. Such diversified phenomena would intrigue any scholarly mind and urge him to seek some order and uniformity in such a welter of diversity. Again the philosophy of the time looked upon man as a single family, recently created, and initially endowed with a high level of civilization. Hence such pagan peoples as were known to scholars of Europe before 1492 were explained as degenerates. But when America was found to be inhabited by a large pagan uncivilized population with no obvious relation to Old World peoples, perplexing questions were raised. As, what was the origin of these Indians? How and when did they appear here and what had been their subsequent history?

Almost from the first, it was recognized that the recovery of this lost history must be achieved, if at all, by natural history methods. What we mean by this is an objective empirical approach, as in inquiries into the history of animals and plants. In the Old World some lost history could be recovered from newly discovered documents and inscriptions on temples and monuments, but in America there was almost nothing of this kind available. At the outset there were skeptics who asserted that nothing could be achieved without written evidence. We are reminded of the famous dispute between Dr. Samuel Johnson and Lord Monboddo.[1] Monboddo believed that lost history could be recovered by natural history methods; Johnson declared that "We can know no more than the old writers have told us." The achievements of geology, paleontology and anthropology have long since proved the absurdity of Johnson's statements. The history of anthropology alone is a fascinating pageant of man's slow but sure recovery of his own lost history. Blundering and faulty research methods there were in plenty, but even from the beginning of the attack upon the Indian problem, we note here and there scientific achievement whenever strict logic was applied even to poor data.

However, our present interest is to review the part the American Philosophical Society played in the development of anthropology in America. It seems that Franklin conceived of our Society as national in scope and urged it to include the best scholars of the country in its membership. While it was inevitable that most Society members should reside in or near Philadelphia, the membership soon included most of the great scholars of the country, so that by the time of the Revolution it was truly representative of the nation. It follows, then, that most of the important contributors to anthropology will be found to have been members of the Society.

It is fortunate that the Society began to publish papers at an early date so that a continuous record is available from 1769. Thus we need not go far afield for a history of anthropology in America. For example in Vol. 4 of the *Transactions* (1799), after stating emphatically that the promotion of research is the main objective, there follows the statement that "The American Philosophical Society have always considered the antiquity, changes and present state of their own country as primary objects of their research."[2]

In keeping with this, circulars were distributed soliciting objects and information of anthropological interest: for example, "To obtain accurate plans, drawings and descriptions of whatever is interesting (where the originals can not be had) and especially of ancient Fortifications, Tumuli, and other Indian works of art, ascertaining the materials composing them, their contents, etc." Information was requested as to the diameter of the largest tree growing on each earthwork, its species, number of annual rings, etc. Further, to inquire into the customs, manners, languages and character of the Indian Nations and their migrations.

[1] Casson, p. 5.

[2] A. P. S., b, V. 4, p. xxxviii.

This makes clear that our Society even in the early days of its existence responded to the presence of anthropological phenomena and took definite steps to carry out its ideal of promoting research and the diffusion of knowledge.

Had Columbus discovered an uninhabited New World, it is conceivable that there would be no place upon today's program for a review of anthropology. An anthropology of a sort there would be, but its configuration would be different. Nor is it likely that this Philosophical Society would have given much consideration to the subject in the face of more pressing questions "concerning the present and past state of the country." However, Columbus discovered an inhabited New World and some thirty years later (1522) the world was circumnavigated, removing any lingering doubts in the minds of scholars that the earth was round and, what is more, enabling them to think of the world as a limited unit of space. To be able to view the world as a whole was no mean achievement; European scholars were the first to realize such a conception and the most impressive aspect of this new world was that it contained a host of savages all obviously related in some way to each other and to the nations of Europe as well.

Knowledge of these savages was sought upon two levels: (1) for practical purposes in trade, government, colonization, education and missionary work; (2) for the sake of pure scence. For example, languages were first studied to facilitate contact with the natives, but later to discover relations between the languages themselves or to solve scientific problems pertaining to them. A missionary might learn a language and even translate the Bible into it without concern about linguistic science; on the other hand he might forget about the saving of pagan souls and engage in the ungodly pursuit of linguistics. Several missionaries were able to do this so successfully as to become great linguists. In this discussion we are concerned with the scientific challenge this savage population made to the European mind, for in response to it came anthropology.

The History of American Anthropology in Outline

In this discussion we should be reminded that we are unavoidably committed to review the history of anthropology in America. So far no adequate history of American anthropology has been written, yet our task has been made relatively easy by two conspicuously able attempts to outline such a history.[3] While these historical sketches are different, one from the other, when integrated, they reveal four major periods in the history of American anthropology, as follows:

1. The exploratory or survey period, 1492–1800.
2. The discovery of research leads, 1800–1860.
3. The museum period, 1860–1900.
4. The academic period, 1900–.

The dates assigned to these periods are somewhat arbitrarily chosen, but should be regarded as focal points in the transition from one period to the next.

It is generally conceded that another way to outline the history of a science is to enumerate the discoveries of basic research leads. In the history of anthropology it is usual to recognize four such leads, as:

1. The discovery of linguistic families or the linguistic method of classification.
2. The comparative anatomical method.
3. The archaeological concept of stratigraphy.
4. The ethnographic approach.

Linguistic Research

The study of languages was the first major objective in anthropological research. Languages are the most distinctly human classificatory characters; man seems to be an animal, to behave much like an animal, but no animal has developed a language of the human pattern, nor has one ever been able to learn and to use such a language. Further the elements of language are objective, susceptible of accurate recording, its symbols and forms can be treated just as impassionately and inhumanly, if you like, as mathematical symbols. As soon as a keen scholar became familiar with many languages, it was inevitable that he discover varying degrees of similarity among them, thus suggesting a basis for classification.

Just who originated the present method of classifying languages is not quite clear. Jonathan Edwards in America in 1787 published a pamphlet suggesting observed relationships among certain Indian languages. He was on the threshold of a great discovery but did not know it. It is usual to regard the German Schlegel as the first to demonstrate the modern method of classification in 1808.[5] Yet in 1708, a century earlier, the lin-

[3] Haven, 1856; Mitra, 1933.

[5] Haven, p. 54.

guist Adrian Reland compared Indian languages with Old World languages, declaring that they showed no relation to the languages of Europe.[6] A quarter of a century before Schlegel made his pronouncement in 1808, Jefferson wrote, "Were vocabularies formed of all the languages spoken in North and South America, preserving their appellations of the most common objects in nature, of those which must be present to every nation, barbarians or civilized, with the inflections of their names and verbs, their principles of regimen and concord, and these deposited in all the public libraries, it would furnish opportunities to those skilled in the languages of the old world to compare them with these, now or at any future time, and hence to construct the best evidence of the derivation of this part of the human race."[7]

Catharine II of Russia is credited with getting the idea at about the same time (1784).[8] She gathered by correspondence vocabularies of languages, even addressing President Washington for lists of Indian vocabularies. She studied her lists for a time, making translations in Russian of words from 200 languages, after which she "threw up her hands" so to speak, and sent out an S.O.S. to the eminent Professor Pallas. It was too big a job for a busy monarch. Nevertheless her initial experiment and encouragement led ultimately to the famous publication *Mithridates,* a cyclopedia of languages to which American scholars made contributions.[9] But Jefferson seems to have gathered vocabularies and made his pronouncement before Catharine made her idea public. Zeisberger, the Moravian missionary in America, was busy with the compilation of his ambitious manuscript of comparative vocabularies and grammatical phrases before Jefferson wrote the lines previously quoted.[10]

So it seems a fair inference that a number of scholars in America were aware of the trend of thought respecting empirical classificatory research in language and ready to profit by the methodological advances in Europe as well as to strike out upon original lines. One important point is to note how forehanded was Jefferson in this scientific movement. He seems to have recognized the trend and to have sought to implement research by promoting the collection of materials. What we can be certain of is that even before 1808 the present system of language classification was taking shape in America. The hope of the time was the conception that a genetic classification might be realized and ultimately result in a kind of family tree leading back to an original language. This hope was so absorbing that the tendency was to confuse the classification with the expected issue, to the neglect of some necessary objectives and methodological safeguards. Albert Gallatin in America seems to have done more to clarify the classificatory method than anyone before his time by carrying the system through to its logical end without regard to the desire for a complete family tree.[11]

All that was needed around 1800 to set scholars off upon a glorious career in linguistic research was the concept of genetic relationship. Darwin cannot be accused of suggesting this evolutionary point of view, for this was before his time. On the other hand Linnaeus cannot escape suspicion because he had thrilled the scholarly world by proposing a system of classification for animals and plants which really worked, but he had to wait almost a century for a Darwin to put life into the scheme. The linguists beat Darwin to it by at least 50 years. Early in 1800 we find Duponceau, a former President of our Society, expressing the need for a Linneaus in linguistics to set up convincing principles of classification. Something like this was achieved by Duponceau and his associates and finally clarified for America by Gallatin, Brinton and Powell, all members of the Philosophical Society.

The Life and Contributions of Peter S. Du Ponceau

As we have just stated, the emphasis at the outset was upon research in Indian languages. The first great American leader in this field was Peter S. Du Ponceau, elected a member in 1791 and President of the Society, 1828–1844. He was a great man in linguistics but a great lawyer and statesman as well. He was born in France in 1760, destined for the army, but becoming near-sighted in early adolescence, was sent to a theological school. Disliking this, he ran away to Paris when 15 years old, where he became secretary to a linguist. He came to the United States as aide to Baron Steuben, serving through the Revolutionary War in that capacity with the rank of major. His proficiency

[6] Haven, p. 56.
[7] Jefferson, p. 163.
[8] Pickering, pp. 321–2.
[9] Adelung, V. 1.
[10] A. P. S., b, V. 3, pp. 65–251.
[11] Gallatin, a.

in languages made him useful to the army. For a time he was assistant foreign secretary in Washington's administration but soon resigned to practice law in Philadelphia.[12]

He had native skill in languages, speaking French, English and German at an early age and also proficient in Latin and Greek. Ultimately he mastered several other European languages. He was the 7th President of the Society. Two of these seven, Franklin and Jefferson, were outstanding as both statesmen and scholars; Rittenhouse, Wistar and Du Ponceau were great scholars. Franklin served 21 years; Jefferson, 18 years; Du Ponceau 16 years. Jefferson was Vice-president and later President of the United States during his term of office as head of the Philosophical Society, and according to the record was present at twenty regular meetings out of 374. He seems to have attended no meetings while he was Vice-president and President of the United States and he frequently wrote letters to the Society requesting permission to resign but no action was taken until 1875, three years before his death. Du Ponceau rarely missed a meeting before becoming President of the Society and as Vice-president, 1818–1828, presided over 250 meetings out of a possible 344. He was a person of wide interests, accomplished in music and literature, a witty and forceful writer. In one of his letters penned near the close of his life, he writes, "I have been a jack of all trades, and as they say, good at none." His achievements belie the pleasantry, but it is characteristic of his modesty, his preference for retirement, rather than publicity.[13]

He believed in the United States, and had an abiding faith in its future. In one of his letters he writes, "I never could bear the despotism and superstition of the Old World." J. K. Kane, a lawyer, wrote (1844) that Du Ponceau "was more American than the Americans." He often expressed the faith that before long the United States would be compensating England by its contributions to the common culture of both. He closely followed all important culture movements in foreign countries and gave legal and intellectual advice to the new Latin-American republics as well as to his own country, becoming a great international lawyer. He persistently advocated closer relations with the Latin-American republics, yet he so loved these United States that above all the many honors he received from abroad, he valued most those conferred by his own countrymen.

If Du Ponceau had one over-all ambition it was to win from Europe the recognition that the United States had produced and would continue to produce good literature, science and scholarship, which was original and not cheap copies of European masterpieces. His mind was strong and clear to the last, though he approached complete blindness and deafness. Possessed of a charming manner, delighting in humorous anecdotes, we envisage him, as a presiding officer, making each meeting of the Society an event.

His bibliography contains more than 40 major titles. He was a member of 43 learned societies, home and foreign. Aside from being President of our Society he was at one time President of the Historical Society of Pennsylvania, President of the Athenaeum, and Founder and President of the Academy of Law.

Work of the Committee on Indian Languages

In 1815 our Society recognized linguistics by creating the Historical and Literary Committee.[14] We do not know who instigated this step but we suspect Du Ponceau with the backing of Jefferson. At the outset this committee chose two objectives, (1) the collection of historic documents and (2) manuscripts recording Indian languages. We suspect that if Jefferson had been questioned as to the propriety of putting so much emphasis on languages, he would have retorted that the lost history of the Indians could be recovered by research in languages. We note that Du Ponceau was Corresponding Secretary of this committee and that a collecting campaign by mail was to be its policy. Success was extraordinary and we attribute this to the intelligence and energy of Du Ponceau. Yet Jefferson set a good example, for before 1780 he began to collect manuscripts on Indian languages.[15] Later he donated 12 important items, among them two of his own, one a vocabulary from the Unguachogs or Patchoag of Long Island. The local tradition is that Jefferson visited the Indian settlement of Pusspatock near Brookhaven to make this record in 1791. That he contemplated a technical paper on American Indian linguistics is obvious. There are some 17 letters written by Jefferson, in

[12] Dunglison.

[13] Heindel; Whitehead.

[14] A. P. S., d, V. 1.

[15] Washington, Jefferson in letter to Barton, V. 5, p. 469.

many of which he urges the collecting and filing of notes on Indian languages, rarely neglecting to state that the problem of Indian origins could best be solved by the intensive study of such materials. About 100 linguistic manuscripts are listed in the collection of our Society as transmitted by Jefferson.

Du Ponceau studied and classified every scrap of language he could obtain. He brought into the Committee the scholarly Moravian Missionary John Heckewelder (1743–1823), who donated at least 24 manuscripts and solicited many others. He and Duponceau exchanged letters discussing technical points in linguistics. The magnitude of this correspondence is evident in 150 or more long letters from Heckewelder, written at the rate of four or five a month. Du Ponceau seems to have written an equal number, the whole constituting an able symposium on Indian languages. In 1836 Du Ponceau received the Volney Prize in Paris, two thousand dollars awarded for the most outstanding contribution upon linguistic methods for research.[16] By this award he was certified one of the few great linguists of the world. Some of his contributions may be noted, such as recognizing the languages of eastern Siberia as of American Indian type and distinct from other Asiatic languages. Modern linguistic research has confirmed this discovery. Further, he identified the Osage language as of the Siouan family. Some insight into the methods of Du Ponceau may be had when we see in the manuscripts in the Society's collections notes and vocabularies copied from books of travel and from original manuscripts borrowed for study.

It is to be regretted that no one has chosen to write a biography of Du Ponceau; he deserves it. His contemporary John Pickering, considered one of the few great linguists of his time, wrote in a eulogy of Du Ponceau that he was "the first to discover and make known to the world the remarkable character which pervades the aboriginal languages of America from Greenland to Cape Horn."[17]

It would be far from just to pass over John Heckewelder's contributions in silence. He was elected a Society member in 1797. Simple, straightforward, thoroughly good, modest and unassuming, yet he soon won the respect of scholarly men in linguistics. Another great linguist of the time was John Pickering (1777–1846) of Boston, elected to membership in 1820. He was a great lawyer, retiring and modest, but a prolific writer. He enjoyed the esteem of Humboldt, Vater, and other great philologists in Europe. He was credited with having studied 27 languages. In 1823, Pickering was appointed to a place on Du Ponceau's committee. From his studies of Indian languages Pickering accepted the existence of four great families east of the Mississippi and Hudson's Bay: the Eskimo, Algonkin, Iroquois and Floridian (Muskogean). To facilitate the recording of Indian and other primitive languages, he devised an orthographic system which was quickly adopted by the Board of Foreign Missions.[18]

Pickering corresponded with William von Humboldt and Du Ponceau, each having a high regard for the others. We get the impression that when in their prime Pickering and Du Ponceau were the world's two outstanding specialists in primitive American languages.

Another master mind was Albert Gallatin (1761–1849), elected a member of the Society in 1791, the same year in which Duponceau became a member. In 1836 Gallatin published a critical resume of American Indian languages. In 1891 J. W. Powell (elected to membership in 1889) published the final great paper on the classification of Indian Languages, which is still the accepted one, or the standard. To quote from Powell: "As Linnaeus is to be regarded as the founder of biologic classification, so Gallatin may be considered the founder of systematic philology relating to the North American Indians. Before his time much linguistic work had been accomplished, and scholars owe a lasting debt of gratitude to Barton, Adelung, Pickering, and others. But Gallatin's work marks an era in American linguistic science from the fact that he so thoroughly introduced comparative methods, and because he circumscribed the boundaries of many families, so that a large part of his work remains and is still to be considered sound. There is no safe resting place anterior to Gallatin, because no scholar prior to his time had properly adopted comparative methods of research, and because no scholar was privileged to work with so large a body of material. It must further be said of Gallatin that he had a very clear conception of the task he was performing, and brought to it both learning and wisdom. Gallatin's work has therefore been taken as the starting point, back of which we may not go in the historic considera-

[16] Dunglison.
[17] Haven, p. 60.
[18] Pickering, pp. 43–360.

tion of the systematic philology of North America."[19]

Likewise Daniel G. Brinton, a contemporary of Powell and a competent linguist, regarded Gallatin as a greater linguist than Du Ponceau.

Linguists of the Museum Period, 1860–1900

We are now approaching the period of anthropological development which we have designated as the Museum Period, 1860–1900. We feel that this designation is justifiable because it is the time when great museums were created and the period in which the greatest contributions to anthropology came from men in museum positions or in bureaus and academies closely associated with museum collections. So far as the materials of anthropology are concerned it was the period of great acquisition. Wealthy men either made great private collections or delegated collecting to employees of museums, thus becoming the collector once removed, as it were. In this way they not only became famous as patrons of anthropology but experienced what they valued more, a detached personal anticipation in the joys of exploration and discovery without suffering any of the necessary hardships and inconveniences. Such collecting reached its climax about 1900 and is now clearly on its way to something else, possibly a period of greater museum use, since the rise of anthropology in universities and colleges is now the dominant note. When some future historian comes to write the chapter about anthropology after 1900 the great contributors will be professors rather than curators. However, we are still considering the contributions of linguists.

The outstanding personalities in the Museum Period are J. W. Powell (1834–1902) and Daniel G. Brinton (1837–1899), both members of the Society. Several other able linguists were their contemporaries. For example, Horatio Hale (1817–1896), elected to membership in 1872.[20] Hale's mother was a distinguished journalist in Massachusetts, credited with having persuaded Lincoln to declare Thanksgiving a national holiday and thus set a precedent, and further the suspected author of "Mary Had a Little Lamb." Hale's first important linguistic paper dealt with the classification of the Algonkin Indian language. He achieved fame as linguist and ethnographer to the Wilkes Expedition. Hale made his living as a lawyer as a citizen of Canada.

Incidentally we note that in 1888 he challenged the value of the Darwinian theory in explaining mental and emotional differences among living races of men. Another unique interest was in the study of child languages; it was Hale who formulated the hypothesis that new groups of savages might be formed from time to time by a few surviving children who would of necessity greatly alter the basic language as they matured. His other contributions were by no means second rate; for example, his discovery that the Tutelo language was Siouan. At his death Brinton praised his work highly, stating that Hale loved science for its own sake.[21]

This citation brings us logically enough to another linguist of distinction, Daniel G. Brinton (1837–1899).[22] He was a great scholar, a master mind, standing high in the medical science of his day, a brilliant linguist and a great contributor to anthropology. We note Brinton's contributions to medicine which we believe to have been considerable. He was a prominent surgeon and Medical Director in the U. S. Army during the Civil War and later editor of the first journal to emphasize scientific medicine. We have been told that his influence in medicine was little short of epoch-making.

Turning now to his anthropological career, we note first that while still a young man he began to make anthropological contributions. His great linguistic ability and the extraordinary drive and energy manifest in the linguistic field culminated in his book, *The American Race*, 1891. Powell's *Classification of North American Languages* appeared during the same year. Brinton was among the first to recognize the value of texts in the classification of native languages and to encourage the collection of them, for much of his work had to do with the translations of such native texts as the Walum Olum, ancient Nahuate Poetry, Rig Veda Americanus, etc. His faith in the value of linguistic classification was strong to the end, as indicated by his expressed belief that ultimately the perfection of classification technique would reveal a single super-American family. His bibliography lists over 70 linguistic contributions.[23]

Brinton's knowledge of Indian languages was so profound that he was able to detect and expose several fraudulent manuscripts. He believed, like Duponceau, that the phenomenon of incorporation

[19] Powell, pp. 9–10.
[20] Goddard, p. 558.
[21] Brinton, b.
[22] A. P. S., c, pp. 210–272.
[23] A. P. S., c, pp. 247–252.

was markedly present in the majority of American Indian languages. He demanded that recognition "be given to the principle of phonetic variation, systematic derivation, of the historic comparison of languages, of grammatic evolution and morphological development," in comparing languages.

We have now followed the history of our Society from the beginning to 1900 insofar as it was associated with research in primitive linguistics. The Society was truly representative of the United States and Canada since its membership embraced all the outstanding contributors to anthropological linguistics. It is apparent that this group of scholars achieved world leadership in the study of primitive languages and, what is more, made it an integral part of anthropology. Du Ponceau dreamed of the day when American scholars should stand shoulder to shoulder with the greatest scholars of Europe; he lived long enough to see this goal realized in primitive linguistics.

Physical Anthropology

It was recognized even at the beginning that comparative anatomy could well serve as a check upon the classification of tribes by their languages. Early in 1700 attention was called to the obvious similarities between Indians and Asiatics. The English portrait painter, John Smibert, came to America in 1739, and as soon as he saw some Indians pronounced them Tartars, having just before leaving Europe made some portrait studies of individual Tartars.[24] Other observers confirmed this resemblance, so that by 1780 all the leading scholars in America seem to take for granted the unity of the Indian race and their affinity with the Mongoloids of Asia. It is true that the literature of the time revealed other theories of Indian origin but the scholars in the Society seemed to sense their lack of merit.

Dr. Samuel G. Morton (1799–1857), elected to the Society in 1823, is regarded as the founder of physical anthropology in America. Many of his contributions are outstanding. Dr. Daniel G. Brinton, who belongs to the next generation (1837–1899), was a firm believer in the importance of this research lead and made substantial contributions, as the list of some 35 titles in his bibliography testifies. Brinton's last contribution to physical anthropology appeared two years before his death.[25]

[24] Barton, p. xvi; Sparks, p. 201.
[25] Hrdlička, pp. 508–554.

Archaeology

Archaeology became a science when the principle of stratigraphy was recognized, but did not come into its own until near the close of the period ending in 1860 and even so did not stimulate systematic research in America until after 1880. Recalling what was said above about the assumed age of man, you will readily understand that the antiquity of man is a recent concept. Fossil man was long considered an absurdity. Even as late as 1846, two years after the death of Duponceau, the discovery of flints associated with extinct animals in the English cavern at Kent was ridiculed. The discoverer was told that such a statement claimed the impossible. He replied, that he had not said it was possible, merely that such association was true.[26] Even in the nineteenth century stone implements were explained in parts of Europe as the result of magic, but the case was different in America. Indians had been seen using such stone tools, so no such foolishness disturbed the learned men of the Society. They readily recognized mounds and earthworks as the work of prehistoric man and similarly regarded ruins in Mexico and elsewhere. They did not consider these as of great antiquity in the modern sense, but still of respectable age. Having reason to believe that the builders were the ancestors of the living Indians and that they migrated here from Asia, they saw no inconsistencies with established beliefs.

The problem of the day was to locate archaeological objects, describe them, classify them and compare them typologically with archaeological objects in the Old World. Some of you may be familiar with the Antiquarian movement and the Society of the Dilettanti in Europe who are now seldom accorded just recognition for the services they rendered. Following this European pattern the American Antiquarian Society was established in Massachusetts in 1812, and at once set a high standard in research. It financed two major projects and otherwise encouraged archaeological research.[27] Our Society was also active: we have noted that in 1799 circulars were distributed inviting records of earthworks and monuments. The wide distribution of these leaflets stimulated local interest and thus contributed to the increase of knowledge.

The Society invited collections; we note among the list of donors that Franklin gave some pottery to the Society in 1789 and Jefferson a stone fig-

[26] Casson, p. 175.
[27] Atwater; Lapham.

urine representing a woman in labor. That the collections grew rapidly is evidenced by the request of the Smithsonian Institution in 1863 for casts of certain objects. In 1864 the Academy of Natural Sciences of Philadelphia voted to loan its archaeological collections to the Society because it led in the quality and size of such collections. Further the *Proceedings* of 1864 recorded a vote of the Society that its policy should be to promote research and collecting in archaeology, regarding their own collections of that date as unusual in size and quality.

After 1870 archaeology made a remarkable growth. As we have said, 1860–1900 was the time of great museum expansion in anthropology, especially in archaeology, and the outstanding leader in this was Frederick W. Putnam. The stratigraphic methods used in Europe were tried out here, but it was not until after 1900 that the American archaeologist Nelson devised a method suitable for American conditions. This lead stimulated an extensive program of research and the perfection of a scientific technique, which made archaeology in America a natural science.

Ethnographic Lead

We turn now to the fourth lead in anthropological research, the concept that the facts of tribal cultures can be interpreted in terms of geography and compared by qualitative methods. As in other instances of this kind it is difficult to identify the originator of the various techniques employed. The first comparative use of ethnographical data coming to our notice is in the argument against "the Lost Tribes of Israel Theory" of Indian origins by the Spanish Jesuit Josef de Costa in 1604, when he asserted there were striking differences between old Hebrew folk customs and the known tribal Indian cultures.[28] On the other hand we know that some historians of European anthropology point to the Swedish scholar John Scheffer's account of Lappland (1673) as a remarkably modern ethnographical publication.[29] About a century later Captain Cook's expedition began to collect museum specimens and to publish first-class ethnographic contributions.

Ethnographic Research

The first American to write an ethnographic treatise to prove a theory was the Indian trader, James Adair (1775), who attempted a rigid comparison of the customs of the Hebrews and of certain southern Indian tribes among whom he lived. He erroneously concluded that the observed similarities in customs were sufficient to prove that the Indians were descended from the lost tribes of Israel, but because he kept his data on Indians separate from his interpretations, scholars still regard the factual part of his book as a classic. He sensed the possibilities in the comparative method of ethnography and the value of meticulously recording the facts. He set a good example in keeping his data and his interpretations separate.

While the great men of our Society were all advocating the collection of data on languages to achieve a genetic classification of mankind, many intelligent missionaries and travelers were recording ethnographic data for its own sake. Such men as DuPratz, Peter Kalm, John and William Bartram, Thomas Say and many others were writing books and papers telling about what Indians did without regard to what use was to be made of these facts. Because of their excellence many of these books were destined to become the classics of later days. Jefferson believed that ethnographic data were worth recording and that one effective way to accumulate the facts was by organizing scientific expeditions. In his memoranda of instructions to Lewis and Clark he gave full directions for the recording of ethnographic data, surprisingly modern in tone, which might even now serve as a guide to a field worker. This document is not famous like Jefferson's draft of the Declaration of Independence but it shows the same masterful grasp of fundamentals, for he fully sensed the modern field worker's job. He went even farther in anticipating the practical value of such knowledge in promoting the acculturation of the Indians. Every graduate student of anthropology should be required to read these memoranda of Jefferson if for no other reason than to knock some of the conceit out of him.[31]

It is clear that the expedition of Lewis and Clark, to which Jefferson contributed so much thought, was to crystallize into a pattern for future expeditions and surveys under government and private auspices. That he blazed the trail for J. W. Powell to establish the Bureau of American Ethnology is not to detract from the great achievements of Powell. There was neither a National Academy nor a Smithsonian Institution in the time of which we write, so the U. S. Government

[28] Mitra, p. 16.
[29] Casson, pp. 118–122.
[31] Thwaites, V. 7, pp. 249–251, 283–287, 408.

often looked to our Society for personnel and guidance. The scientific and scholarly prestige of such unique men as Franklin and Jefferson automatically gave the Society the highest rank in the nation.

That the gathering of information by a competent explorer was ever on Jefferson's mind is evident in the record. He had contemplated exploration west of the Mississippi long before the Louisiana Purchase in 1803. Dr. Goode in his booklet, the *Origin of the National Scientific and Educational Institutions of the United States* (1890), discusses at some length Jefferson's interest in scientific exploration. Thus Jefferson wrote, "In 1792 I proposed to the American Philosophical Society that we should set on foot a subscription to engage some competent person to explore these regions in the opposite direction, that is, by ascending the Missouri, crossing the Stony Mountains, and descending the nearest river to the Pacific." At the same time Jefferson pledged 1000 guineas to such a fund.[32]

It seems that earlier, in 1785, Jefferson induced John Ledyard, a famous explorer, to attempt to reach western America through Siberia, but Ledyard was arrested on the way and returned to St. Petersburg.[33] Jefferson's idea in 1792 was to send a small party from the United States to penetrate the northwestern country. With these acts of Jefferson in mind it is not difficult to understand his eagerness to secure title to the western country by the Louisiana Purchase and his sending out at once an exploring expedition. In the meantime Ledyard died in Africa and Jefferson had encouraged Captain Lewis to prepare himself for an expedition into the Louisiana territory. The emphasis Jefferson placed upon scientific work in his instructions to the Lewis and Clark Expedition will be understandable if one but read the comments of Dr. Goode, also certain passages by Henry Adams in his admirable biography of Gallatin.[34]

Nor did our Society hesitate to petition the U. S. Government to undertake scientific exploration. For example in 1816 a committee consisting of Duponceau and others was appointed to urge a national expedition west of the Mississippi. Noting the date, it is safe to guess that anthropology was to be emphasized in the program. This may have initiated Long's Expedition of 1819 or perhaps merely supported a proposal before Congress. In any case the Long Expeditions attempted to do scientific work of a high type, as evidenced by the contributions of Thomas Say (1787–1834) and of Edwin James (1797–1861). Again when Congress was authorizing the Wilkes U. S. Expedition as a scientific enterprise, the Society was requested to appoint a committee to formulate a plan for the scientific work of the expedition and to select the personnel.[35] Duponceau seems to have been the spokesman for the Society. After due deliberation the Committee recommended that a plan be supplied the Government but that the Society decline to select the personnel. I think we see in this a canny bit of wisdom and the fine hand of Duponceau, who abhorred politics.

In passing we cannot refrain from calling attention to another example of Jefferson's deep insight into the problems of the future. He saw a practical value in ethnographic knowledge, not merely in guiding the administration of Indian affairs but in that the acculturation of the Indians would be more effective if based upon knowledge of the aboriginal tribal culture. These wise counsels of Jefferson were unheeded at the time, to be revived by our contemporaries as ideas original to our own era.

However, the first great leaders in American ethnography were Zeisberger and Heckewelder, whom we have praised for their contributions to linguistics.[36] Heckewelder, especially, regarded the beliefs and mythologies of Indians worth recording as a part of their social histories; he insisted that the Indian was not a beast, that he had intelligence, ability and moral worth. He was violently opposed by certain members of our Society, who considered no Indian beliefs and traditions of value because they were pagan and further because not susceptible of documentary proof. But in the end Heckewelder commanded the respect of a majority of Society members. It is now generally agreed that Heckewelder was a great ethnographic pioneer, who laid the foundation for his successors, among whom Gallatin and Schoolcraft made the main contributions leading to the formulation of the ethnographic lead.[37]

Henry Rowe Schoolcraft (1793–1864) became a member of the Society in 1833. As now, new members were rarely elected until they became

[32] Goode, p. 37.
[33] Goode, p. 36; Sparks, p. 153.
[34] Adams, p. 349.
[35] A. P. S., a, 1837, p. 706.
[36] Zeisberger.
[37] Heckewelder.

famous. He was trained as a geologist and showed competence in that subject. At an early age he manifested an interest in the study of Indian languages and ethnography. In 1822 he was appointed Indian Agent for the Upper Mississippi country. Duponceau praised his work in 1834 and a prize was awarded him for the best essay on the Algonkin language. Philosophical, geological and antiquarian societies elected him to membership and in 1846 the College of Geneva gave him the degree of LL.D. The climax to his career was the authorization by Congress (1847) to prepare a report on the Condition of the Indian Tribes, ultimately published in a series of large volumes, which established the precedent for Powell's post-Civil War Series of Ethnographical Studies, and finally the Bureau of Ethnology in Washington. Schoolcraft conceived the clash of cultures, which was eventually to be realized by the modern school of anthropology. He recognized the reality of a psychological approach. His object was always to submit ethnographical data as "facts or materials" on the Indians for future interpretation. His voluminous reports have been harshly criticized because they contained contributions of unequal value, but they were factual and so deserved a place in such a survey. His own best contributions are to be found in his earlier works.[38]

However, the classical paper making comparative use of ethnological data was written by Gallatin about 1845, four years before his death: "Notes on the Semi-civilized Nations of Mexico, Yucatan and Central America." He discusses at length their languages, their numeral systems and knowledge of astronomy, their history and chronology, finally facing the question as to where their unique culture originated. As to the last he favors independent development; he wants to know why, if this culture was transplanted from the Old World, it was based upon maize and other native flora and not upon the wheat, millet and rice of the Old World. However, he does not stop here, but demands to be shown why "an alphabet, the art of working iron, mills, wheelbarrows and a multitude of other useful arts remained unknown" to these ancient Americans. He wisely adds: "In order to form a correct opinion, it is necessary to take into consideration, not only what the Mexicans knew but also what they did not know."[39] This has a modern ring; students of our time can do nothing better.

What a remarkable mind this man Gallatin possessed! It would seem enough to close the 1800–1860 period in anthropology's history as the author of the outstanding work on linguistic classification, but to add another equally lasting contribution in comparative ethnology is indeed astonishing. We can do no better than quote Samuel Haven who stated that Gallatin "employed a cooler and stricter logic" in handling anthropological data than any of his predecessors.[40]

It remains to speak of Lewis H. Morgan, the first great corporation lawyer, also a statesman of note, but chiefly memorialized as the greatest American social scientist, and often characterized as the Tylor of America. His influence upon ethnography was great, but, though he was a member of the National Academy of Sciences, he was not a member of the Philosophical Society. Strange, for he was about the only great anthropological intellect they failed to capture. Thus we may leave him out with a clear conscience, since Brinton, Putnam and Powell make up for the omission. These remarkable men are sometimes spoken of as the "Big Three" of the period 1860–1900.

Under the head of linguistics we commented upon Daniel G. Brinton's outstanding achievements in that branch of anthropology. Again we noted that the physical anthropologists considered him an important contributor to their field and now we shall find that though Morgan was clearly the greatest ethnographer of the period (1860–1900), Brinton was next in rank. Brinton's faith is expressed as, "anthropology is the natural science of social life." At another time he writes, the aim of anthropology is "to define the universal in humanity," but "we have no right to assume anything universal in humanity until we have proved it."[41]

His most distinctive contribution lay in the study of mythology, beliefs and religion. Like Tylor and Morgan he chose the inductive approach to assemble data, then to classify and integrate them. His grasp of the place of mythology in the wide-flung culture complex of folk beliefs and religion shows him more than a pioneer. Like Tylor he explored the contents of men's minds respecting the outside world, except that he was more psychological than Tylor. In fact he made a bold excursion into the folk-psychological field in his unfinished book, *The Basis of Social Rela-*

[38] Mitra, pp. 69–75.
[39] Gallatin, b, p. 187.
[40] Haven, p. 61.
[41] Brinton, d, p. 8.

tions—a Study in Ethnic Psychology. It is interesting to note that in this his last book Brinton rejected the concept that society was an organism in a biological sense, and in turn rejected the concept of the ethnic mind, or a super-mind; to quote, "the analogy of the group to an organism is purely fictitious; the historic causes of the decay of nations are not the same and not allied to those which bring about mortality in the individual."[42] Like Tylor, he believed in the psychic unity of the human mind. Most contemporary anthropologists profess to believe in this interpretation, too, though many of them seem to have no idea as to what was meant by the phrase.

Space forbids an adequate summary of Brinton's place in the "Big Three" but a characterization is possible. Powell was the director of the world's greatest team of full-time gatherers of field data in all divisions of anthropology; Putnam was a great organizer, a builder of museums and university departments, staffed with men chosen chiefly for their ability in field collecting and museum house-keeping. Like Powell he believed the only place to do research was in the field. Brinton's interests were academic; a scholarly research professor whose vision was turned toward the living functioning of man rather than to the description of museum materials and concrete field data. To put the contrasts in another way, Powell gave chief attention to living man, Putnam to extinct man, while Brinton sought an all-embracing interpretation of culture phenomena.

Above all Brinton was critical. I have heard it said that when an anthropometrist rose to speak, he first looked around to see if Brinton was in the room and shaped his remarks accordingly. At the memorial meeting after Brinton's death W J McGee said: "Brinton's position as a spokesman for a science was curiously like that of the elder Dana in geology, in that both were in chief measure reworkers of raw material produced by others rather than original producers. . . . In one respect Brinton held a unique position among his fellow ethnologists—he was the leading ethnologic critic of the country, if not of the world. . . . It is not too much to say that a considerable portion of American ethnological utterances during the last decade were really addressed to an audience of one, and that one, the fearless critic of Philadelphia."[43]

[42] Brinton, c, p. 40.
[43] A. P. S., c, pp. 244–245.

In reflecting upon Brinton's development we see him beginning with an antiquarian interest, later pursuing the linguistic lead, then turning his attention to the study of mythology and beliefs, and finally seeking the light by psychological methods. Whether by accident or otherwise this is the history of anthropology since 1890. It passed through a revival of the linguistic lead to the intensification of research in ceremonialism and social organization and is now toying with the psychological approach with somewhat less clarity and consistency than did Brinton. Of course this is an analogy and subject to the limitations of all analogies, but I think you will agree that the parallelism manifest in the unfolding scholarship of a great mind and the fused output of a later generation of minds is worthy of more than passing attention.

Achievements of the Leaders in Anthropological Synthesis

When this topic was assigned to the writer, it was suggested that 1875 be considered the terminal date. We have found this difficult because it just misses the climax to a period in the history of anthropology in which museums are the leading institutions. 1890 or 1900 would be the more satisfactory date because those years mark the onset of the academic period in the development of anthropology in America, the period in which specialization and more or less complete isolation prevails to such an extent that our young anthropologists give their whole attention to but one of the four groups of anthropological data—linguistics, archaeology, physical anthropology and ethnology. This is probably inevitable, and what may be more alarming to the older generation, there is now a tendency to sub-divide these divisions both by problems and by regions, threatening thereby to isolate anthropologists one from the other.

But it is apparent that before 1900 the great intellects taking up anthropology strove to be masters of all types of data, seeking thereby to present a synthetic interpretation of comparative human problems, or to answer such direct questions as: How and when did the Indian originate? What is the outline of his subsequent career? Looking back over this review of anthropology and our Society, we note certain conspicuous individual attempts to achieve such a synthesis, as those of B. S. Barton (1798), J. H. McCulloh, Jr. (1829), Alexander W. Bradford (1843) and Albert Gal-

latin (1845). For the museum period (1860–1900) the most conspicuous, sustained attempt was made by Daniel G. Brinton with J. W. Powell a close second. For example Gallatin expressed his regard for archaeology when he wrote "From whatever place the people of America came, the first important question is the time of their arrival.[44] Brinton (1896: 3) looked upon archaeology as essential to an understanding of man; to him man meant nothing except when seen in time perspective.[45]

The first notable attempt to bring all the available data to bear upon the origin and subsequent histories of the Indian tribes was by B. S. Barton. He became a member of the Society in 1789 at the age of 23 and was 32 years old when he published his booklet upon the *Origin and Subsequent Histories of the Indian Tribes* in 1798. His was an outstanding intellect, of a botanist and of a zoölogist. By birth he belonged to the generation of Du Ponceau and Gallatin; he began his career as a physician, trained in Edinburgh and London, achieved distinction in medicine and natural history but died at the age of 49. It thus happens that he became a scientific contributor before Duponceau and Gallatin became conspicuous as anthropological scholars. Interested in botany and medicine, Barton made studies of Indian plant usages, which makes him a pioneer in ethnobotany.

Turning now to his synthetic monograph, we note, first, that greater emphasis was placed upon language than upon other kinds of data. A casual reading of his book will show that he was not a mere copyist but that he compiled special lists of words and phrases to demonstrate his conclusions. He corresponded with Jefferson and Heckewelder and also addressed letters to missionaries and traders on the frontier for vocabularies and phrases in languages familiar to them. In the manuscript collections of the Society are a series of questions sent to Heckewelder by Barton, with replies to them. There are other evidences to show that Barton worked independently with linguistic materials.

Barton assumed that ultimately all New World language would be traced to a single super-family. In this respect he may have been too optimistic but that was the expectation of the time. We have noted that Brinton died in 1899 believing in this possibility. Some of our contemporary geniuses in linguistics still feel that this may be possible.

A contemporary of Dr. Barton was Dr. J. H. McCulloh, Jr., of Baltimore, concerning whom we lack biographical information. His first publication was in 1813, followed by *Researches on America, etc.*, 1817, and later a still more elaborate statement of the case, *Researches, Philosophical and Antiquarian, concerning the Aboriginal History of America*, 1829.

Dr. McCulloh's work is inferior to that of Barton yet has merit. He saw military service in the Ohio county during the War of 1812, which brought him into contact with Indians, earthworks and archaeological collections. He justly criticized the linguistic studies of the time as based upon manuscript data in which phonetic recordings were not sufficiently standardized to be comparable. Further, he was fully aware of the errors frequently made in securing vocabularies from natives. In his review of Barton's book he justly objected to some of the methods used by Barton in the comparison of languages, as: "The common method of tracing a nation or people by means of etymological inquiries appears defective: . . . . It cannot be doubted, that if inquiries could be made by a philologist himself, . . . such a procedure would be highly important; but this plan has not been followed. Navigators and curious travellers have made vocabularies . . . and undertaken to compare them as they wrote, unconscious of the confusion that has been made by the collectors, in labials,—dentals,—or of words or sounds compounded of consonants, and such that an European cannot pronounce. . . . where a language is but imperfectly understood, collectors of words often mistake—by giving the adjective along with the noun; thus—a stone being presented to a savage . . . he might say it was hatchet stone, or spear stone . . . twenty different ways of answering the question and none the direct one; . . . . Another source of error in forming vocabularies is from having a defective ear. . . . The incapacity of etymology to show the origin of the American Indians, cannot be doubted, when we examine the New Views of Dr. Barton."[46]

Dr. Barton's methods were not so crude as McCulloh would have us believe, nevertheless there is justice in the criticism. In McCulloh's final book he challenged the idea that Indians were "copper-colored" since he observed among

[44] Gallatin, b, p. 177.
[45] Brinton, d, p. 3.
[46] McCulloh, a, vii–x.

them white (albinos?) as well as black and brown color types.[47] He thus anticipates Morton in pronouncing brown as the basic skin color. Further, he was aware of the wide anatomical variations to be observed among Indians. However, he regarded the Indians as of one race and the first inhabitants of the New World. He accepted an Asiatic origin but strongly advocated independent development in the New World, on the ground that culture similarities to the civilizations of the Old World were not sufficiently specific to be significant. Had McCulloh possessed a great mind like Gallatin he would have produced the first great classic critique upon the culture origins of the aboriginal civilizations of Mexico and the Andean region to the southwest instead of producing a poorly organized and ineffectual book. Nevertheless he deserves a place in the history of anthropological thought during the period 1800–1860.

Another worthy contributor was a lawyer, Alexander W. Bradford (1815–1867), whose chief publication is *American Antiquities, and Researches into the Origin and History of the Red Race,* 1841. So far as we can learn, this is an "armchair" product but gives evidence of wide reading and critical weighing of the evidence. There are a few minor misstatements of fact in his book but a goodly amount of original interpretation. For example he divided mankind into three great divisions, White, Red and Black, which correspond to the modern terms Caucasoid, Mongoloid and Negroid. Earlier scholars had suggested such a three-fold division of man in the Old World but Bradford seems to be the first to formulate a sound argument for the anatomical affinity of Indians with Mongoloid peoples. He favored the idea that the original Mongoloid migrants from the Old World entered the New World from Siberia and also through the equatorial Pacific. On the other hand he favored the idea that most of the material and other culture achievements of the Indians were independent developments and that the first migrations were from a primitive horizon.[48] As a book, Bradford's volume is superior to that of McCulloh but not so finished and original as the publication by Gallatin in 1845. (See p. 000.)

The unique production of the period 1800–1860 was a comprehensive survey of anthropological achievement by Samuel F. Haven, a great antiquarian scholar of Worcester, Massachusetts, who had not only read the important books and papers on anthropology for the period 1600 to 1860 but who critically considered the evidence presented and in the end enumerated the concepts worthy of acceptance. Space forbids an adequate review of Haven's work but we can list a few outstanding conclusions and insights which we hope will give some idea of what these scholars of the United States had made of anthropology by the year 1860.

1. The great diversity of Indian languages was recognized. Principles of classification were established and the most important stocks identified (Du Ponceau and Gallatin).

2. The Asiatic origin of the Indian was considered established. Further that the peopling of America was by natural sporadic migration at irregular intervals (Haven and Barton).

3. The three great somatic divisions of mankind were recognized as white, red and black, for which we now use the terms caucasoid, mongoloid and negroid (Bradford).

4. The concept of diffusion was expressed in the theory that the centers of highest culture evolved in Middle America and that their influences spread thence into both North and South America (Bradford).

5. Gallatin adequately stated the arguments for the independent development of New World cultures. All subsequent supporting papers have done no more than elaborate his statements, usually without recognizing his claim to priority for the first comprehensive statement.

6. Majority opinion favored a unity of racial and culture traits for the New World as a whole; all the known types of archaeological artifacts were considered ancestral to those in the hands of living Indians in 1492 (McCulloh, Bradford and Haven).

7. The fact that the New World was as old geologically as the Old World should have no bearing upon the antiquity of man in the New (Haven).

8. The significance of geographical distribution was recognized by Gallatin, Haven and others, as for example, noting the regional range of earthworks in general, and the far more restricted ranges of specialized types, as effigy mounds; further, to assert as a principle, that highly localized differences and similarities in artifacts were best explained as local developments, rather than examples of diffusion from distant centers.

[47] Haven, p. 53.
[48] Bradford, pp. 70, 161, 198, 250, 430.

9. A surprisingly modern methodological note is struck by Haven, as when he writes: "Affinities which have no united reference to any particular nation, but point now to one people, and then to another totally distinct from the first, and, in a third case, to others equally disconnected, however numerous they may be in the aggregate, tend, by their diversity to weaken the force of each individual analogy as an evidence of origin, and can only serve to illustrate the possibility of accidental and partial communications. If congruous affinities of a positive character should be found in some detached locality, they might seem to indicate descent from a special stock, but claims to distinctive derivation, founded on such evidence, are opposed by the linguistic and physical proofs of a general unity of race throughout the entire continent." This is quite in line with the modern concept of associated traits and patterns in cultures and the accidental character of scattered similarities in the details of culture. Haven rejected the dogma that all similarities in detail, no matter where found, had a common origin.[49]

We feel that even this inadequate sample of the status of anthropology in 1860 is impressive. In the light of contemporary anthropology there is little for which to apologize and a great deal which was fully confirmed in later years. Through it all the American Philosophical Society, as the outstanding center of influence, furnished the chief medium for publication and housed significant collections of anthropological materials.

As a final summary to this historical sketch we should note that anthropology does not appear as a profession until after 1860 when there was an extraordinary development in museums and government research bureaus. Thus it happens that the first professional anthropological positions fall in the period 1860–1900 and it was not until 1900 that academic anthropology began to get a foothold in universities. The first Ph.D. in anthropology granted in an American university was in 1892. The third such degree was granted in 1898 and the fourth and fifth in 1900. Since this review is expected to cover the history of anthropology before 1880, it will of necessity deal mostly with the work of non-professional anthropologists.

It is often remarked that anthropology is the happy hunting ground for superannuated scholars. This was clearly true before 1880, since until that date most of the noted contributors had previously achieved distinction in some other field of learning. Of course, many weak and sometimes silly papers were written, but the judgment of the Society as to what was admitted to publication was sound, probably as sound as that which now prevails, and the biographies of the authors who published these papers make clear that they were supermen in ability and scholarship. This seems to us sufficient explanation as to how it came about that many significant contributions were made before anthropology became a recognized profession.

We compiled a list of the most important early contributors as follows, classifying them by occupations.

Statesmen:	Thomas Jefferson (1743–1826)
	Albert Gallatin (1761–1849).
Lawyers:	Peter S. Du Ponceau (1760–1844)
	John Pickering (1777–1846)
	Caleb Atwater (1778–1867)
	Alexander W. Bradford (1815–1867).
Clergymen:	David Zeisberger (1721–1808)
	John Heckewelder (1743–1823)
	Jonathan Edwards (1745–1801).
Physicians:	B. S. Barton (1766–1815)
	Samuel G. Morton (1799–1851)
	John C. Warren (1778–1856).
Naturalists:	John Bartram (1699–1777)
	William Bartram (1739–1823)
	Thomas Nuttall (1786–1859)
	Thomas Say (1787–1834)
	Henry R. Schoolcraft (1793–1864)
	Edwin James (1797–1861)
	W. P. Keating (1799–1840)
	J. E. Lapham (1811–1875).
Travelers:	Peter Kalm (1716–1779)
	Jonathan Carver (1722–1780)
	John Ledyard (1751–1788)
	Alexander Mackenzie (1755–1820)
	George Catlin (1796–1872).

Of these twenty-five contributors, fourteen were active members of the Society. Two, Jefferson and Du Ponceau, were presidents of the Society.

Of the period 1860 to 1880, the outstanding contributors to anthropology were:

Lawyers:	Horatio Hale (1817–1896)
	Lewis H. Morgan (1818–1881).
Geologist:	John W. Powell (1834–1902).
Physician:	Daniel G. Brinton (1837–1899).
Naturalist:	Frederick W. Putnam (1839–1915).

Morgan was not a member of the Society but Hale, Powell, Brinton and Putnam were active members.

[49] Haven, p. 149.

As we look backard upon the history of early anthropological thought in America, it appears that all the obviously possible solutions for fundamental questions were proposed. The major papers published when taken as a whole remind one of a debating society, for when one alternative is presented one or more antagonists take the negative. Thus when it was proposed that mounds were built by an extinct race, good arguments were presented to support the view that they were constructed by the ancestors of living Indians. As to the origin of the Indians, all of the possibilities were laboriously presented, as (1) Indians created separately in the New World, (2) migrated from Siberia, (3) migrated across the Pacific in equatorial latitude, (4) across the Atlantic by the northern route, (5) by the equatorial route. There were many who believed that a classification by language would reveal origins; a minority rejected this idea. Again, when someone discoursed upon the impossibility of accounting for the various biological types of man within the time limit of some 10,000 years, some scholarly minds countered with the theory that the environment would see to it that such types came into existence within these time limits. The opposition critically reviewed these arguments by calling attention to the obvious relative antiquity of caucasoid, mongoloid and negroid typyes. Interestingly enough one writer in 1841 put up a good argument in support of the concept that environmental changes affected the individual but were not inherited.[50]

In this review we have tried to note priorities in surviving interpretative concepts, even though they were little more than wishful thinking, omitting references to the many guesses which were rejected. One may well question the wisdom of this procedure. However, in the examples we have cited, the surviving interpretations were the results of long and patient studies of the data available; they have withstood the tests of time because the data upon which they were based presented an adequate sample of the phenomenon in question and because logic was rigidly applied. We have noted how the outstanding men of the period 1800 to 1860 recognized the scientific importance of certain research leads, the logical precision of the methods proposed and the skill of such men as Jefferson, Barton, Duponceau, Heckewelder, Pickering, Lapham, Morton, Hale, Schoolcraft, Bradford, Haven and Gallatin in choosing interpretations which have stood the test of time. In this Jefferson and Gallatin surely lead.

Finally, we hope that this presentation will give an insight into the important contribution the Society made to the development of anthropology not in America only, but in the world as a whole. The late Professor Mitra of Calcutta, commenting upon the history of anthropological research in America, states: "By the time of American Independence, the main research leads in the science of the Indian had been well set. To know the American Indian, to collect ethnographic and sociological data, and to try to solve the problems of origin on a linguistic basis, became the major interests of the scientists who were nucleating round the leading Philosophical Societies and the Academies of the time."[51]

We feel that this statement characterizes the function of the American Philosophical Society. Long may it continue to serve as a center of nucleation for the best scientific opinion.

The writer wishes to acknowledge the assistance of Miss Helen Peltz who kindly volunteered to search the literature for data needed in this historical study. It is to be regretted that but a part of the information collected by Miss Peltz could be used in this brief paper.

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[50] Bradford, pp. 243–247.

[51] Mitra, p. 45.